THE ROLE OF POLITICAL INSTITUTIONS IN ECONOMIC DEVELOPMENT
AN EMPIRICAL INVESTIGATION

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THE ROLE OF POLITICAL INSTITUTIONS IN ECONOMIC GROWTH

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Some goods are only valuable, and some investments only profitable, in the presence of certain institutions. The empirical literature on institutions and economic growth claims that institutions are a primary determinant of growth. I find four mechanisms through which institutions may affect growth in Africa: property rights, contract enforcement, security and corruption. This dissertation consists of three papers that empirically analyze the importance of these mechanisms while addressing the major empirical issues of endogeneity, measurement and level of analysis.

The first paper uses Afrobarometer surveys to measure distinct regional institutions: fear of crime, law enforcement, trust and corruption. I combine these variables with household data from 151 regions in thirteen African countries. I avoid endogeneity by regressing household wealth on aggregate institutional variables, and I find that reducing the fear of crime increases wealth and improving the legal system increases wealth when fear of crime or trust in the national government is high. Corruption in different levels of government has positive and negative effects. The results suggest that variation in institutions within countries is important.

The second paper (with Christine Moser) explores institutions within Madagascar at a low administrative level. We use a unique commune census to analyze the impact of institutions and infrastructure on development of the manufacturing sector in Madagascar. We find that not only do institutions matter, they play a causal role in both employment in manufacturing and in infrastructure. The data is a spatially explicit panel countrywide census with reasonable instruments for institutions. We account for bias due to unobserved heterogeneity, endogeneity,

and omission of neighboring commune characteristics. Our results suggest that property rights institutions are fundamentally important for economic development in Madagascar.

The third paper analyzes democracy in Africa. Africa has become steadily more democratic since the end of the cold war, and I identify three mechanisms through which democracy may be instrumentally good. Democratization is associated with better institutions, which lead to economic growth. Democracies are more accountable and thus may have better health outcomes, and democratic countries have open political systems that may obviate civil conflict. I estimate the effects of democratization with instrumental variables (IV) and simultaneous equations models. I find no significant effects of democracy in the IV models when country and time fixed effects are included. The simultaneous equations results, which also control for country and time fixed effects, indicate that democracy may have strong effects on economic growth and significant but weak effects on the death rate in African countries.

BIOGRAPHICAL SKETCH

Alan Green was born and raised in Bristol, Tennessee, where he was Valedictorian at Tennessee High School in 1999. He enrolled at Furman University, where he graduated Summa cum Laude with a Bachelor of Arts degree in Political Science in May 2003. Alan spent the next year at the University of Chicago earning a Master of Arts degree in International Relations in May 2004. He then enrolled at Cornell University as an M.S. / Ph.D. student in the department of Applied Economics and Management. Alan passed his Ph.D. "A" Exam in April, 2008.

This dissertation is dedicated to my wife, Christina, and our children, Al, Johnny and Gracie.

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LIST OF ABBREVIATIONS

SSA	Sub-Saharan Africa
AJR	Acemoglu, Johnson and Robinson (2001)
GDP	Gross Domestic Product
ELF	Ethno-linguistic Fractionalization
DHS	Demographic and Health Surveys
IV	Instrumental Variables
WDI	World Development Indicators
MEPV	Major Episodes of Political Violence
CID	Center for International Development
OLS	Ordinary Least Squares
GLS	Generalized Least Squares

Chapter 1

An Introduction to Institutions and Economic Growth

1. Introduction

Some goods are only valuable, and some investments only profitable, in the presence of certain public goods and/or institutions. These include public goods ranging from roads and electricity to institutions such as property rights and contract enforcement mechanisms. Such institutions are typically not accounted for in a neoclassical framework, but they make up the key underpinnings of markets. Their fundamental importance is sometimes overlooked in developed countries where they are readily provided, but in the developing nations of the world it is critical that researchers and practitioners alike take into account the role of institutions and public goods in development. Without them, market based reforms are unlikely to work, and a "big push" of aid can similarly be expected to fail.

Several concepts here need to be clearly defined. The definition of a public good – one that is nonexcludable and nonrival (and sometimes indivisible) - is standard (Cornes and Sandler 1986, Kimenyi 2006). I will further differentiate between pure public goods, which are fully nonexcluudable and nonrival, and club goods, which are excludable (Cornes and Sandler 1986). The definition of institutions is somewhat less clear. The new institutional economics school vaguely defines institutions as "rules for interaction" (North 1990). I interpret this definition as the written or unwritten guidelines that guide people's expectations of behavior. Institutions can be formal, such as explicit contracts, laws, etc., but they also can be informal rules such as table manners and social etiquette. Institutions underlie neoclassical economics, but they are typically assumed to function perfectly.

Microeconomic theory posits an implicit set of institutions. First of all, property rights must be defined and enforced (i.e. there is little or no theft, and individuals know what is tradable). Second, the neoclassical framework assumes relative equality and complete security – there is no coercion involved. Third, there

must be enforceable contracts, particularly for intertemporal trade and for managing risk.

A set of institutions can be seen as a public good, typically provided by a government. For example, good governments provide security; they protect people's bodies and property. They have clear laws that define what is and is not property, what rights people have, etc., and they provide courts to enforce these laws and contracts. I therefore define good governance as the steady provision of a set of institutions. My focus here is on institutions and growth, so institutions are considered to be good insofar as they allow for economic growth. The question of which institutions best encourage economic growth will be considered in detail in further chapters, but the basic answer is those institutions that are assumed to function perfectly in a neoclassical economic model: stable property rights, effective and anonymous contract enforcement, security and lack of corruption.

Institutions are often taken for granted in economics. This is probably because rich countries typically enjoy a stable set of homogenous institutions governing economic transactions, so there simply is not much variation in institutions in developed countries. In developing countries, however, institutions are far less homogenous. Security is often an important issue, property rights are often not well defined, contract enforcement mechanisms may be weak and corruption may be rampant.

It is clear that as rich countries have developed, institutions have become more stable and homogenous, but it is not clear which way the causality runs. Does improvement in institutions allow growth or does economic growth lead to better institutions? (Paldam and Gundlach, 2008). It seems likely that causality runs both ways. The natural question then is what does the evidence say? At this point, the

evidence on institutions is far from conclusive, and this dissertation will seek to add to it in several different ways.

This introduction will develop a framework for thinking about institutions and growth, review the current literature on institutions and economic development and lay the foundation for the subsequent three chapters, each of which will add to the empirical literature on institutions. I will focus on sub-Saharan Africa. The countries in this area have similar enough histories and levels of development to make talking about them together sensible (Gordon and Gordon 2001). This area of the world also lags behind everywhere else in terms of economic development and especially growth, so research that gives insight into these processes in Africa is certainly needed.

The next section will outline a theoretical framework for thinking about institutions in Sub-Saharan Africa. Section three then reviews the cross-country empirical literature and section four the micro and meso empirical literature. Section five introduces the subsequent three chapters.

2. Theoretical Framework

Rather than attempting to posit one unifying theoretical framework, I will try to paint a broad picture by reviewing several relevant models that touch on aspects of public goods and institutions in Africa. The first model is the standard public good model, including both pure public goods and club goods. The standard result then follows that government intervention to provide the pure public good from tax revenues is the efficient outcome. In the case of the club good, it can be provided privately or publicly in efficient quantities; in either case the key factor is the criterion for exclusion. A market-based criterion will lead to efficient outcomes, while exclusion based on some other criteria, such as ethnicity or membership in a patronage network can lead to inefficient outcomes.

The question now is: to what extent is the allocation of public goods in sub-Saharan African (SSA) countries efficient? To answer this, and to justify theoretically our focus on SSA, I turn for a moment to a political theory of state formation (Herbst 2000). The basic idea presented in Herbst (2000) and echoed in later works by Robinson and Parsons (2006) and others is that the countries we see in Africa today are fundamentally different entities than those that evolved in Europe or anywhere else in the world. Herbst argues that Africa in pre-colonial times was land abundant, so there were few wars over territory and poorly developed land rights. People, by contrast, were the relatively scarce commodity and therefore property rights over people were much more developed (however abhorrent we may find them today). This basic situation in the eighteenth and nineteenth centuries in Africa is contrasted with Europe, where competition over the scarce resource – land – led to the development of strong, consolidated states with clearly defined territories. The important point is that strong states arose out of a situation of fierce competition for territory (Herbst 2000).

When the European powers colonized Africa, they found some powerful kingdoms and tribes, but few well defined borders (Abernethy 2000). They therefore created borders, in a fashion that paid little or no heed to the ethnic/tribal groups living there. These borders were retained at independence, and have changed very little since then (Gordon and Gordon 2001). Thus the countries we see in Africa today were created not through a competitive process that rewarded strong, centralized governments, but rather through arbitrary line drawing on a map by the European powers (Abernethy 2000). The result is that many African countries do not and cannot effectively assert authority throughout their territory (Herbst 2000). They also tend to be divided into many distinct ethnic groups, rather than forged along ethnic lines as in Europe. Herbst's argument suggests that were some countries to become predatory,

Africa would experience some wars over territory and would in turn develop stronger states. However, in the postcolonial period the international community has recognized and given aid to the existing countries while foisting strong norms of sovereignty throughout the world (Barkin 1998). Thus any African state that attempted outright predation would be ostracized and perhaps even stopped by outside forces. The international system strongly supports the status quo states in terms of territory (Barkin 1998). International norms of sovereignty and non-aggression explain the small number of interstate wars in postcolonial Africa.

Therefore, largely as a result of outside forces, African countries today typically are characterized by weak governments, in the sense that they cannot assert authority throughout their territory and often experience crises of legitimacy (Herbst 2000). Since they face no territorial threats and the countries are divided along ethnic lines, African governments tend to build their support from one region or ethnic group. When they ascend to power, they then need only to reward their supporters and hold on to power internally. Without external threats, one simple way to do so is through systems of patronage that reward supporters. A typical African country in this framework will have one group that comes to power and then focuses all of the resources of government on rewarding their supporters and maintaining power (Herbst 2000). This type of "neo-patrimonialist" government is contrasted with the modern "legal/rational" states we see in the developed world (Robinson and Parsons 2006). A neo-patrimonialist state generally maximizes the surplus it can extract from non-supporters to give to supporters and also rent-seeks international aid. It is accountable only to supporters, who have little incentive to increase overall efficiency. Out of power groups may have such incentives, but they tend to either be successfully dominated or to manage a coup d'etat only to act the same way themselves. This theory thus provides an explanation for the large number of civil wars in Africa. In

contrast, in a legal/rational state, one government successfully asserts its power over its entire territory and thus provides a set of homogenous institutions to its constituents. Legal/rational states are thus characterized by strong centralized governments, homogenous institutions and stability, while neo-patrimonialist states are characterized by weak governments, heterogeneous institutions, and instability (Herbst 2000, Robinson and Parsons 2006).

We have now outlined a broad theory of the problems of African states. Two points need to be made before we move on. First, there are exceptions to the theory. Robinson and Parsons (2006) suggest Botswana as the notable exception. Other countries fit to varying degrees, but I argue that the theory applies generally enough to SSA to be useful, and Robinson and Parsons (2006) echo the sentiment that this theory is widely accepted among political scientists. The second point is that this particular theory applies uniquely to Africa and does not broadly fit other areas of the developing world. Only Africa has the unique combination of postcolonial states with arbitrarily fixed borders ruled by indigenous people. Latin America does not have the same degree of arbitrary borders, nor has power there ever been handed back to indigenous people. Asia, with its higher population density had more effective states before the colonial period, and parts of Asia also had territorial wars at independence and in the postcolonial period, further consolidating state power (Abernethy 2000).

Now that we have brought to mind the standard public goods problem and also laid out a political theory of states in Africa, it is time to combine the two to generate some hypotheses. Our political theory suggests that African governments are likely to be dominated by one ethnic group that probably sits atop a patronage network. Kimenyi (2006) argues that throughout Africa this type of situation leads to two public good outcomes. The first is that pure public goods will be underprovided by the government because they benefit all groups and the government maintains power more

effectively by channeling resources to its own group. The second outcome is that club goods will be provided to members of the group in power, but not to members of other groups. Indeed, club goods may actually be overprovided to members of the group in power (Kimenyi 2006). The criterion for group membership is usually ethnic identity, which strictly limits entry and exit of groups. Thus the public goods outcomes predicted here are quite inefficient.

Our framework thus far suggests that one group in power will transfer resources back to members of that group while ignoring and/or repressing other groups. The next question is what happens to those people who are mostly ignored by the government. These people can expect very little in the way of public good provision, and also very little in the way of police protection, contract enforcement and property rights. Economic theory in general does little to model outcomes under such "lawlessness." Dixit (2004), however, in his book Lawlessness and Economics, does model several possible outcomes where the government is not present. His models formalize some of the observations of Fafchamps (2004), such as producers/traders bringing goods to port and sleeping with them because that is the best way to insure against robbery. Dixit's models also capture some aspects of the work of Platteau (2000), who argues that social norms are very important in developing countries.

We will not fully present Dixit's models here, rather I will summarize the intuitions and results of several of them that seem relevant and again generate some hypotheses. The basic idea behind all of these models is the prisoner's dilemma. People stand to gain from cooperation in trade, in provision of public goods, and in security, but without either trust or enforcement the incentives to cheat are too high for cooperation to occur.

The first model captures this intuition in a simple framework dubbed "relation versus rule based governance." (Dixit 2004) This is a game where a continuum of players are positioned on a circle. When any two players interact they play a prisoner's dilemma game. There are two mechanisms that allow for cooperation. The first is social sanctioning, which is effective in small groups. This is modeled as each player having a neighborhood around them of people they trust and will cooperate with, while outside of this neighborhood they will not cooperate. The intuition is that reputation mechanisms can insure cooperation in small groups, villages for example, where interactions will be repeated, if not with the exact same player then with someone else who knows everyone's reputations. The second mechanism is a system for anonymous monitoring, similar in intuition to the credit rating system in the U.S. Here any player can pay a cost to learn the type of any other player. This system allows for efficient outcomes in large groups, but is generally too costly to implement in small populations. Thus there are several equilibria possible. For small populations, the reputation mechanism is efficient, and for large populations, the credit system is efficient. An interesting result is that as one moves from a small population to a large population there is an intermediate range where neither system is efficient. This model thus suggests that in the absence of government provision of contract enforcement we should expect reputation mechanisms to be very important, but it is an open question whether formal or informal mechanisms are most efficient (Dixit 2004).

A second set of related models considers private provision of public goods. In these two models we see private provision of information services and enforcement services. The first models information services. Here we again have players playing a prisoner's dilemma, but now someone (aptly dubbed "Info") collects information on other players and sells it for a fee. In the second model we again have prisoner's dilemmas, but here someone (dubbed "Enfo") can be paid to inflict punishment. The

results of both of these models are that private provision can improve efficiency to some degree, but there are also risks that Info and Enfo will attempt extortion or be dishonest themselves (Dixit 2004).

Finally Dixit models a society where individuals have three choices: they can be producers, protectors or bandits. If a government exists that provides protection, there are no bandits in equilibrium. If a government is not present then an equilibrium has an equal number of all three occupations, which is clearly inefficient. However, he also considers the possibility here that the government itself is predatory. Then it is necessary to distinguish between roving bandits and stationary bandits. Roving bandits take whatever they can and move on; all that can be done is attempt protection. Stationary bandits, on the other hand, stay in one place and therefore do have incentive to invest in productivity. Thus the equilibrium with a stationary bandit is more efficient, if still far short of the optimum situation of good governance (Dixit 2004).

These models give us some intuition of what to expect in situations where formal/federal government is absent or is predatory. In some cases we will see that they do reflect what is observed empirically. In other cases we don't have good empirical evidence, so these models offer a starting point for further analysis.

One last theoretical note is to briefly review the analysis of corruption. Our political model posits a nepotistic government, so corruption is likely to be present and potentially problematic in many African countries. Bardhan (1997) reviews the literature on corruption and emphasizes the following theoretical results: Corruption can theoretically decrease efficiency by imposing transactions costs and poorly allocating resources, but it can also theoretically increase efficiency in cases where bribes allow one to bypass red tape or distortionary policies. The net effect of corruption is therefore an empirical question, and one that is currently in dispute. The World Bank has sought to root out corruption in lending countries and Easterly (2006)

argues that governments in developing countries are so corrupt that aid given to them will be wasted. Sachs (2005), on the other hand, claims that governments in Africa are reasonably good given their level of development and that corruption is not a big problem. Based on our expectation of weak, nepotistic governments, we should expect that corruption in African countries decreases efficiency in most cases.

This theoretical discussion has sought to bring together aspects of political and economic theory and introduced several models to provide a framework for the subsequent arguments. The key point from this discussion is that I expect to see large variation in institutions both across and within African countries. The across country differences will arise from the different sizes and ethnic divisions of these countries. The within country differences will arise from the general failure of African governments to effectively assert power throughout their territory. This variation is in contrast to developed countries, which are characterized by a much more homogenous set of institutions within and even between countries.

The purpose of this dissertation is to further our understanding of how variation in institutions, both across and within countries in Africa, affects economic growth in Africa. There are four primary institutions that may be important for growth: property rights, contract enforcement, security and corruption. All four can lead to inefficiencies and all four may be problematic in African countries.

The best system of property rights (I claim, without proof) is a homogenous set of property rights for every good that people want to trade in an economy. These rights need to be upheld by well-enforced laws. Homogeneity allows for maximum trade among the largest group of people, which can lead to optimal economic growth. However, my framework for Africa suggests that such homogeneity is highly unlikely. Consider land titling: my framework posits countries where the power of the government does not extend everywhere inside the borders. It is therefore likely that

areas without a strong government presence will have their own systems of land rights, while more centralized areas may have formal legal deeds to land as is commonplace in developed countries. The lack of a homogenous system, coupled with potentially higher volatility in traditional systems, can then hinder economic activity with regards to land, thus preventing investment and inhibiting growth.[1]

Contract enforcement is necessary for an economy to develop because contracts enable trade over time, provide stability to workers and employers and reduce the risk of many transactions. An efficient outcome is a system of perfectly enforced anonymous contracts, and again I expect most countries in Africa to be far short of this optimum. Court systems based on the principle of anonymity are an anathema to patronage systems of government, whose operating principle is that those in the network are treated differently than everyone else. Consequently, I expect large inefficiencies from poor contract enforcement, which may inhibit growth.

Security is a public good that is generally taken for granted in developed countries. However, basic insecurity, meaning a high risk of robbery and/or murder, can lead to costly adjustments by economic actors. If robbery is expected if one is alone on the road to market, people will only travel in large groups and by daylight, thus limiting their options and increasing the cost of getting goods to market. More broadly, if insurgent groups form to take on the government, the resulting civil conflict can grind economic activity to a halt. I discussed previously how the interstate borders in postcolonial Africa have been remarkably stable, but the arbitrary nature of those borders has resulted in unstable governments and civil conflict in many cases. Security is therefore an important institution to consider in Africa.

[1] See Pande and Udry (2005) for an example of a traditional system of land rights that caused economic inefficiency.

The framework I presented suggests that African countries are likely to be run as patronage systems, which are inherently corrupt. Such systems clearly lead to inefficiency by rewarding connections in the system over merit. It is therefore likely that corruption may be highly problematic for economic growth in Africa. It is also possible, however, that some corruption at lower levels can actually increase efficiency by allowing people to bypass red-tape. The net effect of corruption is therefore an empirical question, and an important one.

This dissertation will empirically analyze the role of institutions in economic growth in Africa. The theoretical framework identifies four key institutional mechanisms to focus on: property rights, contract enforcement, security and corruption. It further provides a broad political and historical explanation for why these institutions may be heterogeneous within and across African countries. The next step is to review the current empirical literature and identify the key empirical issues that arise when analyzing institutions and growth.

3. Cross-country empirical results

Cross-country studies on institutions have been widely discussed in recent years (Pande and Udry 2005). The basic idea of these studies is to carry out cross-sectional regressions on GDP with some measure of institutions as an independent variable. However, institutions are clearly endogenous to GDP, so the literature has focused on finding plausible instruments for institutions. Perhaps the best known papers on these lines are Acemoglu, Johnson and Robinson (hereafter AJR, 2001) and Acemoglu and Johnson (2005). AJR find that settler mortality rates from the eighteenth and nineteenth centuries are actually a plausible instrument for modern day institutions. Using this instrument they subsequently find that property rights institutions are positively and significantly correlated with GDP (AJR 2001). Acemoglu and Johnson (2005) further consider contracting institutions, but find that

property rights institutions are more important. The argument for the plausibility of settler mortality as an instrument hinges on the claim that institutions are historically path-dependent (AJR 2001, Pande and Udry 2005).

Sokoloff and Engerman (2000), in another well-known paper, argue similarly for the path-dependence of institutions. They compare the Caribbean and South American colonies with the North American colonies in the eighteenth century, noting that the Caribbean and South American colonies at that time had much higher per capita income but also much greater inequality than the North American colonies. They then argue that the institutions in the Caribbean and South American colonies were set up to maintain inequality and that this limited subsequent growth, whereas the more equal North American colonies were much better positioned to take advantage of the industrial revolution and grow (Sokoloff and Engerman 2000). Obviously this is a great simplification, but in this seminal paper two key points emerge. The first is that institutions do seem to matter; the second is that institutions seem slow to change over time.

Other papers have also tried to find good instruments for institutions and these are surveyed in Pande and Udry (2005). I will not discuss them further here except to note that there has been much contention over the role of geography in these models (Pande and Udry 2005). Acemoglu, Johnson and Robinson (2001) show no significant effects of geography on GDP when they control for institutions, and they subsequently claim that it is unimportant. Sachs takes exception to this, and claims to show that if one broadens the sample used by AJR, then geography does have significant effects in its own right (Sachs 2003). Rodrik, Subramanian and Trebbi (2005) find that the effects of institutions "rule" over the effects of geography and trade integration. Others try to use some geographical variables as instruments for institutions, further complicating the debate (Pande and Udry 2005). It is clear that

most measures of institutions are correlated with geographical variables, but it is not clear how important the direct effect of geographical variables are.

The literature has established that institutions do seem to matter, but the use of instrumental variables techniques in this literature generally limits the regressions to static estimations and limits the institutional variables to one (Pande and Urdy 2005, Rigobon and Rodrik 2005). Even Rigobon and Rodrik (2005), who use the unique indentification through heteroskedasticity estimation technique to get around the endogeneity problem, use only two institutional variables. Their estimations also may suffer from selection bias. The upshot is that this literature has not progressed much further than asserting that institutions matter for growth. Empirical questions of which institutions matter and how they change over time have not been addressed

It is worth briefly discussing here what types of measurements of institutions are being used. There is a fair amount of data available on institutions of governance at the country level.[2] These data usually are some form of index of corruption, democracy, rule of law, etc. They are most often drawn from surveys, but the surveys are often of businessmen, usually foreign, doing business in the country. There are several issues with such data. The first criticism is that they often capture a foreigner's perspective on particular institutions, which can be subjectively biased especially against perceived corruption (Bardhan 1997). This criticism leads to the second, stronger one, which is that all of these data are of a subjective nature, if not in the survey questions themselves then in the creation of indices out of these questions (Kremer 2004). Kremer (2004) points out that these measures do not give us any idea of where the government is present and absent in a country and what happens when it is absent. Kurtz and Shrank (2007) also argue that such measures fail the basic requirements of being representative of the population of interest and relevant to the

[2] http://www.worldbank.org/wbi/governance/data.html

question at hand. Possibly because of these limitations, the literature does not discuss the institutional data in great detail. AJR (2001), for instance, use a measure of the probability of extraction as their favored variable, but they emphasize that the results hold with several other institutional variables as well.

The cross country literature in general accepts that institutions matter for long-term growth, but this view is not without criticism. Paldam and Gundlach (2008) review the evidence and argue that it is more likely that growth causes improvements in institutions than the other way around. Clark (2008), argues that the institutionalist explanation for growth does not fit with historical data. These authors lean towards the view that institutions arise endogenously as an economy grows, but that they are not likely to be fundamental forces that keep economies from growing. Avoiding the problems caused by this endogeneity in cross country regressions was the original motivation of AJR; however, it is possible that other factors may have biased those IV results.

Interestingly, a new complication to this literature comes from a recent paper by Acemoglu, Johnson, Robinson and Yared (2008). In "Income and Democracy," they focus solely on the effect of national income on democracy, and they find that when country fixed effects are added to a panel estimation this income effect becomes insignificant. While their results do not challenge earlier institution papers directly, the fact that introducing country fixed effects changes the results so drastically suggests that any estimation without country fixed effects may be underspecified.

Pande and Udry (2005) point out that the nature of the cross-country approach – finding plausible instruments for institutions, usually in variables like settler mortality or some measure of geographical distance, limits the interpretation of the results. These instruments are generally fixed in time, so in using them we cannot gain any insight into the evolution of institutions themselves and they cannot be

included in a fixed effects estimation. Also, the instruments are correlated with many of the available institutional variables above, but given the paucity of instruments researchers are limited to only using one or two institutional measure in any instrumental specification. Thus the results conclude that institutions matter, but we gain no insight into the mechanisms through which they matter or to the relations between different institutions within a country. I now turn to the micro and meso level empirical evidence.

4. Empirical Evidence at the micro and meso levels

We will begin with the "meso" level evidence, where meso in this framework simply means within country but not at the individual or micro level. One important issue here is price transmission – to what degree do price changes at the macro level affect people in different areas, specifically those in rural areas? Moser, Barrett and Minten (2005), using a unique dataset from Madagascar, find that price transmission there is actually very low. They attribute this to high transport costs and poor integration of local markets into regional and national markets. Part of their explanation for high transport costs is poor infrastructure. Pinstrup-Andersen and Shimokawa (2008) review the literature on rural infrastructure and conclude that massive investment in infrastructure in developing countries is needed. They argue for international aid to fund such initiatives because infrastructure, a public good, is systematically underprovided in developing countries. They also point to the problems of low price transmission and high transport costs due to lack of infrastructure. However, they do not discuss in detail possible explanations for the lack of infrastructure and their solution of funding infrastructure projects through foreign aid may be of limited use (Pinstrup-Andersen and Shimokawa 2008). Bates and Humphreys (2005) model provision of public goods by an elected government and argue that electoral accountability is key for public good provision in Africa.

The emphasis on accountability ties into a large literature on democracy and growth. Schmitter and Karl (1991) argue that accountability is one of the defining characteristics of democracy, and Gerring, Bond, Brandt and Moreno (2005) argue that a country's "stock" of democracy is critical for growth. Rodrick and Wacziarg (2004) also argue that democracy is good for growth, but the cross-country evidence is limited. Gerring, Bond, Brandt and Moreno (2005) do not account for endogeneity and their result depends on an arbitrary "interest rate" for democracy. Bates (2006) finds that accountability does matter, but also that democratic transitions may lead to instability. Van de Walle (1999) looks carefully at democratic transitions in Africa and finds no consistent link between short-term democratic transitions and economic outcomes.

There is therefore a claim of a general trend that infrastructure is systematically under-provided in African countries, possibly due to lack of accountability of the government. These arguments are in line with my expectations, but the theory predicted more specifically that pure public goods would be underprovided and club goods would be provided to supporters of the group in power. Kimenyi (2006) offers some interesting evidence along these lines. Drawing largely from Brockerhoff and Hewett (1998), he presents 11 African countries that have had long stable periods with rule by one or two ethnic groups. In nine out of these eleven, the ethnic group(s) of the ruler(s) had a significantly higher percentage of women attending school. Also in nine out of eleven, the ethnic group(s) of the ruler(s) had a significantly higher percentage of children receiving complete immunization. There was only one country, Rwanda, where an ethnic group other than the ruler's had significantly better outcomes than the ruler's group (Kimenyi 2006). However, the ruling Hutus make up ninety percent of the population of Rwanda, while the Tutsi minority was highly favored in terms of education during the colonial period.

Kimenyi (2006) also presents evidence, again from Brockerhoff and Hewett (1998) that in five of the countries above where they had data, rural communities dominated by the ethnic group in power had significantly more "all weather roads." In six countries where data was available, rural communities dominated by the group in power had significantly smaller median distances to public health facilities. Taking this into consideration, Kimenyi (2006) argues that schools, immunizations, health facilities, and in some cases roads are club goods, and this evidence thus supports our theory that club goods will be provided along ethnic lines by the group in power. While this evidence supports my theoretical framework, it is still limited. More work could certainly be done along these lines.

Rather than looking at which ethnic groups are in or out of power, Miguel and Gugerty (2004) take a different approach and uses a measure of ethnic diversity (ethno-linguistic fractionalization, or ELF). They test whether or not ELF affects the funding for schools and the maintenance of wells in rural Kenya. While the national government covers most of the costs of Kenyan schools by paying teachers' salaries, local communities are responsible for the costs of books, materials and any expansion projects. Their hypothesis is that more ethnically diverse communities will be less successful at coordinating to raise such funds. The data from 100 schools in two districts in rural Kenya support this hypothesis. They find that increased ethnic diversity significantly reduces the total funds raised by the local community. Miguel and Gugerty (2004) attribute this affect to greater difficulty in coordination and social sanctioning across different ethnic groups.

While Miguel and Gugerty's (2004) argument is interesting, it is not clear that ELF is the best measure of the effects of ethnicity, simply because it is not diversity itself that drives the results, it is people's belief that such diversity is a hindrance. Indeed in their paper Miguel and Gugerty (2004) mention some interviews with

headmasters that gave the impression that the headmasters saw working with different ethnic groups as problematic. Miguel (2004) himself, in a subsequent paper, does a similar analysis with a rural district in Tanzania. Here he finds that ethnic diversity has no effect on local school funding or well maintenance. Miguel (2004) then compares this district in Tanzania with one of the ones from Kenya. He claims that they are similar in climate, ethnic variation, level of development and history except that one is in Kenya and the other Tanzania. He therefore attributes the insignificance of ethnic diversity in Tanzania to the cumulative effects of President Nyerere's nation-building policies that began immediately after independence. In this case he mentions some other interviews with locals in Tanzania, and notes that they said that ethnicity does not matter in Tanzania (Miguel 2004).

Thus while we have some empirical evidence on ethnicity, it remains a complicated task to determine when and where ethnicity matters. At a national level, the differences highlighted by Kimenyi (2006) are interesting, but we are also interested in local effects. Here the limited evidence we have is mixed, and it seems that the effects of ethnic diversity cannot be generalized; rather they are specific to an area. Thus it may be better to try to measure directly the institution of interest – be it property rights, contract enforcement, or democracy, rather than using data on ethnicity because it is available and arguing that it explains the institutional results. Put another way, this is to interpret Miguel and Gugerty's (2004) finding not as "ethnic diversity matters," but as "we observe community level variation in local public good provision, and this variation is correlated with ethnic diversity."

One last point of interest here is that both Miguel and Gugerty (2004) and Kimenyi (2006) point to some studies on the United States for evidence when discussing provision of local public goods and ethnic diversity. While these studies may be helpful methodologically, I do not give any credence to a comparison of these

factors between African countries and the United States. As Miguel (2004) shows, the effects of ethnic diversity are not consistent even among two regions with similar climates, regions of the world, and histories. We should not expect them to be the same across the U.S. and SSA.

There is also some evidence on the role of contract enforcement, property rights, and social norms in Africa. Fafchamps (2004) considers these issues in detail in his book, and offers evidence from several case studies of African firms. He makes the following conclusions. First, the probability that two parties will trade is higher if they have traded before. This is simply a more formal way of saying that relationships matter when contract enforcement is not perfect. He next argues that when two parties are strangers, they will likely only transact in cash, on the spot. Moving on to more structural issues, he claims that there are more intermediaries between producer and consumer in African countries, and further that these intermediaries capture a large share of the gains from trade. Fafchamps (2004) points out that trade in Africa is often more market-based than trade in developed countries, in the sense that it consists of a series of spot market transactions. In developed countries, hierarchal exchange within firms is more common and is organized over a longer time frame with a smaller percentage of transactions actually taking place in a spot market.

Fafchamps' (2004) conclusions all point to the fact that formal contract enforcement is problematic in Africa. His surveys of traders give some indication of this, as they cite many contractual problems and stress the need for flexibility in contracts (Fafchamps 2004). However, these surveys are of businesses that are actually functioning and they therefore give no indication of how many potentially beneficial transactions do not take place because the costs due to the uncertainty of contract enforcement are too high. His surveys also suggest that businessmen rarely go to court to settle disputes, although they do sometimes resort to legal action

(Fafchamps 2004). This result is hard to interpret. Dixit (2004) models transactions "in the shadow of the law," where even with an effective court system, an efficient equilibrium only requires a small percentage of disputes to actually go to court. Here it is the reliable threat of going to court and getting a fair outcome that matters. Fafchamps' (2004) surveys cannot tell us what peoples' expectations were when they went to court, or whether the court was biased in some systematic way.

Jean-Phillippe Platteau (2000) provides some micro-level examples in his book. While he does not present systematic or representative evidence, he does provide enough examples and counter-examples to convince us that studying norms and institutions is a difficult process. He argues as well that when formal contracting mechanisms are absent people will transact on the basis of reputation and knowledge built through repeated interactions. He also confirms the predictions of our theoretical framework that Africa in general has a legacy of ineffective governments (Platteau 2000).

Pande and Udry (2005) also present some evidence from Ghana in their review piece. They have data on land holding institutions in Ghana. To summarize, land there is "owned" by the community, and leaders are charged with distributing land to people. However, they are expected to do so on the basis of need, and to not leave anyone out. There were several examples in the data where leaders had failed to distribute land to those who were poor and subsequently lost their high positions. Thus redistributive norms are highly prevalent there. The problem, as characterized by Pande and Udry (2005), is that the leaders do not have perfect information on who is actually poor and who is not. There is thus a perception among the people that someone who leaves a field fallow must not be poor simply because they can afford to leave their field fallow. The result from their data is that people who are less well connected and worse off tend to not leave their land fallow, even when doing so would

greatly increase productivity. Indeed, the authors estimate that one third of the possible productivity of the land is lost due to failure to let it lie fallow (Pande and Udry 2005). The equilibrium that results is thus inefficient in production.

There are no easy solutions to the inefficiency found in the land tenure system in Ghana. The market solution would assign titles to the land and then establish a market where these could be bought and sold, thus ensuring that the full productivity of each plot would be realized. However, as Dixit (2004) notes, attempts to put a market system of land rights in place where a traditional system already exists have largely backfired because many people do not recognize the rights as defined in the new system. They still rely on the traditional system, and the result can be direct conflicts between the systems as a few people switch to the new but most still rely on the old. What follows then is confusion and conflict rather than the efficient outcome expected by the theory. Difficulty in finding appropriate policies is a consistent theme in this literature (Fafchamps 2004).

My theoretical model points to the possibility of civil war as different groups vie for the recognition and rents that accrue to the formal government. Indeed post-colonial Africa has had a large share of civil conflicts.[3] However, it is not surprising that there is very little data on countries that have had conflicts. In one case, Sierra Leone, there is a post-war survey that reports the degree of violence in different regions (Bellows and Miguel 2007). Bellows and Miguel (2007) have begun to use this to test some basic hypotheses about the amount of violence in a region and its subsequent standard of living. However, all they have found so far is that in regions that experienced greater violence more people are politically active and more people are religiously affiliated (Bellows and Miguel 2007). We do not have any idea what the long term effects of such conflict will be.

[3] See the Correlates of War datasets: http://www.cow.org

Perhaps of more direct policy interest is finding ways to prevent conflict. Miguel, Satyanath and Sergenti (2004) use rainfall in Africa as an instrument for growth and then analyze the effects of negative growth shocks on the probability of civil conflict. They find that a five percent negative growth shock increases the likelihood of civil conflict by one-half. Their paper is an improvement over the earlier literature led by Collier and Hoeffler (1998), which did not take into account the endogeneity of civil conflict, but many questions about the causes and costs of civil conflict in Africa remain.

In a review of empirical evidence that has thus far emphasized the limited nature of the results and the myriad difficulties in estimating institutional effects and finding appropriate policies, I shall conclude with a more positive review of what Robinson and Parsons (2006) dub the exception to the theory of neo-patrimonialism: Botswana. Botswana is exceptional in Africa for its consistent growth, for its stable government, and for its successful management of its natural resources. Robinson and Parsons (2006) point out that Botswana has accomplished all of this despite being a low-population density African country, thus seemingly defying the theory outlined above of weak states due to low population densities and no competition for territory (Herbst 2000). However, it is clear in their own review of Botswana's history that several factors set it apart even before the colonial period (Robinson and Parsons 2006).

Before they were formally colonized in the late nineteenth century, the Tswana people were already organized and protecting their land and interests from the threats of the Boers and the British in South Africa (Robinson and Parsons 2006). Thus they already established some common identity by the time they were colonized, and they maintained an identity through colonization. At independence, the Tswana then played a large role in shaping the subsequent institutional set up of the new nation.

They integrated smaller groups into their coalition, and benefited from a consistent set of shared interests: almost everyone in the country was involved in cattle-ranching at that time. Thus the government actually represented all of the people, as opposed to other countries that saw the establishment of governments partial to a particular group. The Tswana-dominated government then protected the interests of its constituents, but in this case the constituents were the entire nation and almost all had similar interests and needs (Robinson and Parsons 2006). Thus I would argue that Botswana's success is not the perfect counterexample to our theoretical expectations, it is simply an African nation with reasonable territorial boundaries and few ethnic divisions. Its policies have therefore been productive and served its people well.

Of particular note in Botswana is how the country has avoided the resource curse (Robinson and Parsons 2006). Large diamond deposits were discovered in the early 1970s, and these have been the base of a steady industry ever since. The government has made sure to invest the surplus from the diamonds back into infrastructure and other productive activities, and they have also negotiated favorable terms with De Beers that protected Botswana's interests (Robinson and Parsons 2006). Botswana also scores highly on most measures of governance, especially compared to other African countries. In a measure of regulatory structure, Botswana actually ranks higher than Portugal and France (Robinson and Parsons 2006). Although these measures are limited, it is encouraging to know that one African country that has created effective institutions is one that has been stable and successful.

The literature on institutions in Africa is at a point where it has been established that institutions are important, but we have very little indication of which ones matter relatively more or less, the mechanisms through which they develop and change, and the degree to which they can even be compared across regions and countries. Policy prescriptions are even more troublesome; Dixit (2005) points out

that literature in several areas related to institutions suggests diametrically opposed policies and Fafchamps (2004) emphasizes the great difficulty of prescribing simple changes to complex processes.

Much work remains to be done to understand how institutions in Africa relate to economic growth. More empirical evidence is needed, but any attempt to clearly identify the effects of institutions must account for the endogeneity of institutions to economic outcomes, address the many problems in the measurement of institutional variables and determine a level of analysis where the results can inform policy. This dissertation seeks to contribute to the empirical evidence on institutions in Africa. My hope is to improve our understanding of how institutions relate to economic growth and to provide policy advice for African countries.

5. Introduction of the subsequent chapters

The three subsequent chapters will each consist of a paper that analyzes institutions in Africa with different data and at different levels of analysis. Chapter two uses regional measures of institutions created from Afrobarometer surveys, which allow for analysis of institutions within African countries at one administrative level down from the national government. Using Afrobaromter surveys allows me to construct new measures of institutions, specifically focusing on security, law enforcement and corruption. These regional institutional variables are then combined with household wealth measures from DHS surveys. By using a household dependent variable with regional institutional variables I am able to avoid the endogeneity problem that plagues the cross country literature.

Chapter two shows that regional variation in institutions matters in Africa. Specifically, fear of crime is costly and improving courts is generally good, but the interactions matter – improving courts is most effective when crime is seen as a problem and people trust the government. Improving courts is detrimental if there is

not a crime problem and there is no trust of the government, especially in non-urban regions. The analysis also shows the dual facets of corruption; it can be harmful at higher levels but helpful at lower levels of government. The chapter establishes that institutions do vary within African countries, as expected, and that this variation is important for household wealth.

The third chapter, written with Christine Moser, takes the analysis of institutions to a much lower level within a single country. The general consensus is that institutions matter at the country level and chapter two shows that they matter at the regional level as well in Africa. Chapter three looks even lower, at the commune level in Madagascar. There are over 1500 communes in Madagascar, akin to counties in the U.S, and we use a unique data set from Madagascar to analyze institutions in Madagascar's communes.

We show that institutions do matter at such a low level and we further consider the effects of subjective security, public goods and formal property rights on the development of manufacturing in the communes. After testing for fixed effects and controlling for endogeneity and spatial bias in a large sample, we find that formal property rights are the key driver of both the development of manufacturing and the level of infrastructure in Madagascar's communes.

Chapter four returns to country level analysis. In it I make several methodological improvements to determine the country level effects of democratization in Africa. I introduce an instrument, the weighted average of neighboring countries' Polity scores, which allows for IV estimation including time and country fixed effects. I also estimate a simultaneous equations model that includes time and country fixed effects.

I find that when fixed effects are included, institutional variables are clearly insignificant in cross country IV regressions. However, the simultaneous equations

model indicates a strong effect of Executive Constraints on both economic growth and GDP per capita in Africa. I also find that Polity significantly reduces the Death Rate in African countries. The simultaneous equations model gives a test of the overidentifying restrictions of the model, and this test indicates no evidence against the restrictions. I thus prefer the simultaneous equations estimates to the IV estimates because they account for all endogenous variables. The results indicate that democratization may be very good for growth in Africa.

This dissertation begins with the empirical result that institutions seem to matter for growth and a theoretical framework as to how they might matter in Africa (AJR, Acemoglu and Johnson 2005, Herbst 2000). In three papers, I investigate four possible mechanisms through which institutions may affect growth – property rights, contract enforcement, security and corruption – at three different levels of analysis. Table 1 broadly summarizes the results.

Table 1: Summary of Estimated Institutional Impact on Growth at Different Levels

	Local level (chapter 3)	Regional level (chapter 2)	Country level (chapter 4)
Property Rights	Significant Estimated effect on development of manufacturing	Not measured	Significant estimated effect in simultaneous equations model
Contract Enforcement	Not significant	Significant estimated effect of expectations of law enforcement	Not measured
Security	Not significant	Significant estimated effect of fear of crime	Not significant
Corruption	Not measured	Significant estimated effects of corruption in police and reps.	Not measured

I find that property rights institutions have significant positive estimated effects on growth at the local level and the country level. These results are consistent with much of the literature on institutions and growth, and the variables and estimation techniques in chapters 3 and 4 are also similar to the literature. The regional level analysis in chapter 2 uses new institutional variables and a different identification strategy, and the results there are quite different from the other chapters. I find that contract enforcement, security and corruption all have jointly significant estimated effects on wealth in a nonlinear specification. Unfortunately, I do not measure property rights at the regional level, contract enforcement at the country level or corruption at the local or country level. It is therefore impossible to determine at this juncture whether the differences in estimated effects at different levels stem from the identification strategies, the way the institutional variables are constructed, or the omission of certain pathways at different levels.

This dissertation has analyzed institutions and economic growth at three different levels and begun to investigate which institutional mechanisms are the most important. However, more work is still needed, particularly at the regional and local levels where data is scarce. I have been as rigorous as possible, but the data used in each chapter still has significant limitations. New data and analysis is needed to test further the results shown here and other hypotheses about institutions and economic growth.

Chapter 2

Institutions Matter, but in Surprising Ways:

New Evidence on Institutions in Africa

I. Introduction

It has long been argued that certain political institutions are necessary for economic growth, (North 1990) and recent years have seen many attempts to quantitatively show that institutions matter (Acemoglu, Johnson and Robinson 2001, Rodrik et al. 2004, Rigobon and Rodrik 2005, Bates 2006). However, cross-country regressions are mired in problems of endogeneity with very few instrumental variables, and quantitative micro-level evidence on institutions typically comes from small samples and is not generalizable (Pande and Udry 2005, Fafchamps 2004). Thus the literature has on the one hand country-level studies that assert that institutions matter but cannot identify which ones and on the other hand micro and meso level works that point to specific institutions – property rights, contracts, crime, trust or corruption – but do not have representative samples. This paper brings both parts of the literature together. I use household wealth with regional institutional variables to identify the effects of multiple institutions on wealth in a large representative sample covering thirteen African countries.

This analysis confirms the assertion of the country-level literature that institutions matter, but also shows that within country variation in institutions is important. It further tests the effects of several institutions. I find that reducing fear of crime is beneficial for wealth, while improving the legal system is beneficial if fear of crime or trust in national government is high. Corruption in different levels of government has positive and negative effects on wealth. The effects of institutions are nonlinear and there are significant differences between urban and non urban regions.

The structure of the paper is as follows: Section II provides background on macro and micro/meso work on institutions. Section III discusses in detail the data on institutions used here, measurement issues and the estimation strategy. Section IV presents the results and Section V discusses them in detail. Section VI concludes.

II. Background

Although interest in the economic effects of political institutions is a longstanding question in economics (North 1990, Becker 1969, Becker and Stigler 1974), quantitative research on institutions is a relatively recent phenomenon. Sparked by the works of Acemoglu, Johnson and Robinson (2001, hereafter AJR) and Sokoloff and Engerman (2000), economists have begun an attempt to econometrically identify the effects of institutions in cross-country regressions. The main estimation problem is endogeneity, so most of the literature concerns itself with finding appropriate instruments for institutions (AJR, Rodrik et al. 2004, Bardhan 2005, Pande and Udry 2005). However, good instruments are rare, and the results are often difficult to interpret. The use of instruments generally limits the regressions to static estimations and limits the institutional variables to one (Pande and Udry 2005, Rigobon and Rodrik 2005). Even Acemoglu and Johnson (2005), who make use of all available plausible instruments, use only two institutional variables. The upshot is that this literature has not progressed much further than asserting that institutions matter for growth. Empirical questions of which institutions matter and how they change over time are difficult to address in cross-country research.

Micro and meso level research on institutions has identified some of the institutions that may be important. The importance of property rights is a common theme from broad macro-level work (Bates 2006, AJR) to surveys of African traders carried out by Fafchamps (2004) and Fafchamps and Minten (2001). The latter two works emphasize that relationships and trust are very important in economic transactions among African traders. Fafchamps and Moser (2003) as well as Fafchamps and Minten (2001) emphasize the negative effect of crime on trade – not always through measurable theft, but through the costly adjustments made by individuals to avoid theft. These works all also discuss the importance of contract

enforcement. The court system is very important for the threat of legal action, even if few disputes are actually settled there. Dixit (2004) models this enforcement "in the shadow of the law," and both Dixit (2004) and Fafchamps (2004) refer to a long literature on the importance of contract enforcement for economic development.

Other research argues for the importance of infrastructure for growth. Pinstrup-Andersen and Shimokawa (2006) argue that infrastructure is systematically under-provided in African countries. Moser, Barrett and Minten (2005) explore how transactions costs due to poor infrastructure in Madagascar lead to poor price transmission and low market integration, and Kimenyi (2006) argues that public goods such as roads and schools have in many cases in Africa been distributed along ethnic lines. Bates and Humphreys (2005) model provision of public goods by an elected government and argue that electoral accountability is key for public good provision in Africa.

The emphasis on accountability ties into a large literature on democracy and growth. Schmitter and Karl (1991) argue that accountability is one of the defining characteristics of democracy, and Gerring, Bond, Brandt and Moreno (2005) argue that a country's "stock" of democracy is critical for growth. Rodrick and Wacziarg (2004) also argue that democracy is good for growth, but the cross-country evidence is limited. Gerring, Bond, Brandt and Moreno (2005) do not account for endogeneity and their result depends on an arbitrary "interest rate" for democracy. Bates (2006) finds that accountability does matter, but also that democratic transitions may lead to instability. Van de Walle (1999) looks carefully at democratic transitions in Africa and finds no consistent link between short-term democratic transitions and economic outcomes.

Finally, corruption has received a good deal of attention in the literature. Bardhan (1997) offers a review, and emphasizes that theoretically corruption can

decrease efficiency by raising transactions costs, but it can also increase efficiency by allowing individuals to bypass distortionary policies. The World Bank has sought to root out corruption in developing countries and Easterly (2006) argues that governments in developing countries are so corrupt that aid given to them will be wasted. Sachs (2005), on the other hand, claims that governments in Africa are reasonably good given their level of development and that corruption is not a big problem.

The quantitative literature on institutions and economic development is still in the early stages. The cross-country literature, relying heavily on IV methods, confirms that institutions matter for growth, but cannot tell us which ones or how. Many other works point to specific institutions: crime, contracts, property rights, accountability and corruption, but the evidence is either from small samples or from questionable cross-country regressions. This paper begins to fill in the questions of which institutions matter and how they affect economic outcomes with quantitative evidence from nationally representative surveys in thirteen African countries.

III. Methods and Data

What are institutions and what can we measure?

The empirical literature on institutions is vague with regards to what institutions are and how they can be measured. I will thus attempt to offer some clarity before proceeding. Institutions are rules that form people's expectations of others' behavior (North 1990). The economic study of institutions focuses on how different rules affect economic outcomes. For example, it is widely accepted that a set of rules defining what is property and respecting ownership of property (even against the government) are beneficial for economic efficiency and investment.

The key to this definition is that institutions are rules, which presents difficulties in terms of measurement. Informal rules/institutions cannot be directly

measured; formal rules can in theory be measured directly. In practice, what matters is how the formal rules/laws are applied, and it is unlikely that the formal reading of a rule will map exactly onto its application. Thus formal rules present measurement difficulties as well, particularly in a developing country context where law enforcement may be ineffective, as is the case for most of the African countries studied in this paper.

While rules cannot be reliably measured, outcomes and expectations can. However, such measurements link only indirectly to the institutions themselves, which is an unavoidable problem in this type of research. Outcome based measures have the advantage of being objective but the disadvantage of being noisy. For example, one can measure the levels of various public goods, the pay and education level of judges, or the number of murders or robberies in a society, which are all outcomes that depend on the institutions of governance and public good allocation. However, these outcomes map very imprecisely back to the institutions themselves. The level of public goods tells us nothing about the rules of allocation, higher judicial pay and education cannot tell us if there are biases in a legal system and lower crime rates themselves give no indication of who provides security and what measures individuals take to avoid crime.

Some surveys thus measure expectations directly. These measures have the advantage of being less noisy but the disadvantage of being subjective. They capture peoples' perceptions of how others will behave, which can then be linked back to institutions. For example, consider the institutions surrounding basic security. If a society is secure, all people follow the rule of not harming others, which then leads to the expectation that people do not harm each other. If the security is perfect, then both the objective measure (no crime is reported) and the subjective measure (people do not expect to be harmed) give an accurate measure of the institution. However, consider

the situation where objective crime rates are low but not zero. These rates could arise because security is good and almost everyone follows the rule of not harming others, but low rates could also arise if most people engage in opportunistic crime. If everyone follows the rule: commit crime opportunistically (i.e. steal when it is easy), the expectation is that others will steal from you if you do not protect your goods, so everyone makes costly adjustments. The result is that objective crime is still low, but only because people always travel in large groups, lock their doors and do not leave goods unattended. Objective measures of crime cannot differentiate between these two situations, but subjective measures can. This distinction is important because the economic implications of the two different security institutions are vastly different.

Subjective measures thus have some advantage over objective measures. It is critical, however, that subjective measures capture the perceptions of people who are relevant to the institution and the economic outcome and that they are representative of the population of interest. Many of the subjective indicators of institutions that are commonly used in the literature do not meet these criteria of being representative and relevant. The governance indicators constructed by the World Bank[4] and the indices of legal formalism used by Acemoglu and Johnson (2005) rely on the perceptions of "experts" (i.e. lawyers or scholars) who are in no way representative of the population of interest (Kurtz and Schrank 2007, Kaufmann, Kraay and Mastruzzi 2005). Furthermore, many of the commonly used institutional measures are some form of index that has an arbitrary weighting system, which introduces further noise into the measurement.

I therefore develop subjective expectations-based measures of institutions from Afrobarometer surveys that capture relevant institutions, are representative of the

[4] The world bank indicators are available at www.worldbank.org/wbi/governance/govdata/

population of interest and avoid arbitrary indexing.[5] Afrobarometer surveys are nationally representative surveys that ask questions about democracy, corruption, governance, etc. The Afrobarometer surveys are stratified into regions/provinces and then further into urban and rural areas of each region/province. This stratification allows for the construction of regional variables that are representative due to the random selection of observations at the regional level. I created regional variables from the individual responses in the Afrobarometer surveys and matched them to household data from Demographic and Health Surveys (DHS) in thirteen African countries where data were available. Each Afrobarometer variable corresponds to the perceptions of a percentage of the population of a region.[6]

Fear of Crime

In the example above I presented two possible security institutions: do not harm others and commit crime opportunistically. Since opportunistic crime leads to costly adjustments, it will have a negative effect on wealth. Fafchamps and Minten (2001) and Fafchamps and Moser (2003) point out that crime is problematic in Africa for precisely this reason. The Afrobarometer surveys ask people how often they fear crime, with the possible responses being never, rarely, sometimes, a lot or always. This forms the first regional variable, Crime, which measures the percentage of people in a region who fear crime rarely, sometimes, a lot or all of the time. This variable measures peoples' expectations of opportunistic crime. The first hypothesis follows naturally:

[5] See www.afrobarometer.org

[6] See appendix 1 for a complete description of how the variables were constructed and a comparison with other measures of institutions. Afrobarometer data can be found at www.afrobarometer.org DHS data can be found at www.measuredhs.com

Hypothesis 1: Living in a region where fear of crime is higher will result in lower household wealth. The sign of the estimated coefficient on Crime should be negative.

This first hypothesis reflects my general expectation of the effect of Crime on wealth, other things being equal. However, it is possible that the levels of other variables may affect the impact of Crime on wealth. For example, Crime may have a smaller impact on wealth in places where law enforcement is good and corruption among the police is low and vice versa. Crime also may have larger effects in urban areas. I will therefore empirically consider interactions of Crime with other institutional variables and the effects of Crime in urban and non-urban areas.

Contract and Law Enforcement

Fafchamps (2004) refers to a large literature that argues that contract enforcement is important for development. Indeed, a fundamental part of market economics that is often taken for granted is the ability of anonymous individuals to engage in intertemporal trade. Such transactions require institutions of trust on the parts of the individuals and enforcement in the case of breach or hold up. Dixit (2004) models how the effectiveness of courts matters even when few disputes ever reach a courtroom; it is the legitimate threat of going to court that improves contract enforcement.

The Afrobarometer surveys contain three questions that pertain directly to law enforcement. The best of these asks people how likely it is the law will be enforced if someone obtains goods or services without paying, but this variable is not available in all Afrobarometer surveys. The other two, which are available in all surveys, ask how likely it is the law will be enforced if someone does not pay their taxes or if someone commits a serious crime. Law-tax is the percentage of people who think it is very likely the law will be enforced if someone does not pay their taxes, and Law-crime is

the same if someone commits a serious crime. The second regional variable, Law, is a weighted sum of Law-tax and Law-crime.[7] This variable captures people's expectations of law enforcement, which measure the degree to which institutions of formal law enforcement are present. It is increasing in the expectation that the law will be enforced.

> Hypothesis 2: Living in regions with better systems of law and contract enforcement will result in greater household wealth. The estimated coefficient on Law should be positive.

As with the Crime variable, hypothesis two reflects a general expectation of the effect of better law enforcement on wealth. However, the effect of Law may depend on the levels of other variables so I will once again consider interactions of Law with other variables such as Crime.

Trust

The fundamental point made above is that anonymous individuals need to be able to engage in economic transactions for markets to function efficiently. Another way to measure how well institutions in a region allow this is by asking people about their general level of trust of people they know, of strangers, and of the government. The expectation is that the higher the general level of trust, the better the institutional framework is for anonymous transactions. General levels of trust are expected to correlate positively with effective systems of property rights and contract enforcement.

There are five measures of trust that I used from the Afrobarometer surveys: how much people trust the army, the courts, the police, their national representative, and their local representative. The first four of these are highly correlated, so I use

[7] The weights were derived through factor analysis. See Appendix 2 for a description of the factor techniques and the loadings for each factor.

Trust Army and Trust Local Rep as the third and fourth regional variables.[8] These simply measure the percentage of people who trust the army or their local representative a lot. They capture peoples' expectations of the general level of trust.

Hypothesis 3: living in a region with higher levels of trust will lead to greater wealth. The estimated coefficient on both trust variables should be positive.

Corruption

Corruption is likely to be present in most African countries, and the World Bank has put together programs to fight corruption. However, economic theory shows that corruption could have negative or positive effects on wealth (Bardhan 1997). Paldam and Gundlach (2008) aptly dub these two effects the "cost effect," which is the negative effect of increased transactions costs due to corruption, and the "grease effect," which is the positive effect of corruption enabling beneficial transactions to take place. The net effect of corruption is therefore an empirical question. I generally expect that corruption in African countries decreases efficiency because governments are unlikely to be strongly enforcing distortionary regulations.

There are three relevant questions on corruption in Afrobarometer: how people view the level of corruption in their representatives, judges, and police. The corruption measures in representatives and judges are highly correlated, so I only use corruption in representatives and police in the regression. The fifth and sixth regional variables, Reps Corrupt and Police Corrupt, measure the percentage of people who think that some or all of their representatives or policemen are corrupt. These are measures of peoples' expectations of corruption.

[8] Using any one of the first four trust measures did not change the results in any significant way

Hypothesis 4: living in a region with higher levels of corruption decreases household wealth. The estimated coefficients on Reps Corrupt and Police Corrupt should be negative.

As with the other institutional variables, the effects of corruption on wealth may depend on the levels of Law or whether the area is urban. Hypothesis four thus represents my general expectation of the effect, but I will consider interactions as well. Table 2 summarizes the Afrobarometer variables.

Table 2: Summary Statistics of Afrobarometer Variables

Institution	Variable	What it measures	Hypothesis	Mean	Std. Dev.
Opportunistic Crime	Crime	Percentage who fear crime at least a little	Negative effect on wealth	0.33	0.17
Contract / Law Enforcement	Law	Percentage who think it very likely the law will be enforced for taxes and serious crimes	Positive effect on wealth	*0.00	1.00
Trust	Trust Army	Percentage who trust national government a lot	Positive effect on wealth	0.34	0.31
	Trust Local Rep	Percentage who trust local reps a lot	Positive effect on wealth	0.42	0.22
Corruption	Reps Corrupt	Percentage who think some or all reps are corrupt	Negative effect on wealth	0.60	0.18
	Police Corrupt	Percentage who think some or all police are corrupt	Negative effect on wealth	0.40	0.17

*The mean of Law is zero because it is a weighted sum of two variables; see Appendix 2.

Endogeneity

Endogeneity is the fundamental econometric problem in this type of analysis. The most common techniques to avoid endogeneity are to use instrumental variables (IV) and/or to use lags of the independent variables. I take a different approach here which is to identify the effects of institutions through a multilevel specification. This

section defines the problem of endogeneity and discusses the differences in using lags, using IV methods and the multilevel strategy used here.

Endogeneity is often referred to as a problem of reverse causality (Acemoglu and Johnson 2005), but the term reverse causality suggests that causality either runs from institutions to growth or from growth to institutions, or possibly both. In reality, what we have are governments that make choices. Institutions are typically thought of as being chosen or at least being directly affected by the choices of governments, as are the economic outcomes of interest. Thus both institutions and growth are endogenous to the governments' choices.

The argument that lags of an independent variable are exogenous even if the contemporaneous variable is endogenous follows from the reverse causality logic. If the problem is that growth in the present may cause institutions in the present, a natural argument is that growth in the present cannot cause institutions in the past. However, when the problem is cast as one of government choices affecting both institutions and growth, this logic breaks down. Government choices in the past were made with consideration of the future in mind, so both growth in the present and institutions in the past are impacted by government choices in the past. In general, the choices of the government in the past affect both institutions and growth in the past and in the present, so using lags to avoid endogeneity fails.

There may be cases where there has been a regime change such that the choices of the past government are exogenous to present outcomes. In that case data from the old regime could plausibly instrument for the present institutions. This argument leads to the IV method.

The IV method consists of finding some clearly exogenous variables that meet an exclusion restriction, which says that they affect the dependent variable only indirectly through their effect on the endogenous variable. This restriction is not

directly testable, so the success of an instrument hinges on the plausibility of the argument that the exclusion restriction is met. There are very few plausible instruments for country level institutions, which limits what can be identified through this method. At best, two institutional variables may be included in an IV regression of growth or GDP on institutions (Acemoglu and Johnson2005).

Since lags are not exogenous and instruments are rare, this paper uses a multilevel identification strategy that consists of regressing a household level measure of wealth on household controls and aggregate institutional variables. The assumption necessary for this strategy to succeed is that households treat institutions as exogenous to their choices about consumption and production. [9] Since institutions are typically thought of as being chosen by governments at a highly aggregated level, this assumption is relatively innocuous. [10] It is similar to the standard economic assumption that individuals are price-takers, even though prices are jointly determined by the actions of all individuals. It is thus arguably far less demanding than the typical exclusion restrictions required for IV estimation, and this strategy has the further benefit of identifying multiple institutional variables and their interactions.

Household Data

The drawback of the multilevel strategy is that the results are the effects of institutions on household wealth rather than on national growth. However, policies

[9] The concern is often raised that this technique is equivalent to regressing regional means of the household variable on the regional variables and thus is still endogenous. See appendix 3 for a proof that this is not the case since household controls are used.

[10] One can imagine a model where it is not. For example, if households choose where to live based on a vector of institutions at each location, then the resulting institutions could arise endogenously due to household selection into different locations (the "voting with their feet" idea). For this model to make sense, one needs moving costs to be low, which implies that locations are close together, that other considerations (like wages) change little across locations, and that people are free to move. None of these conditions are met in the data used here: institutions vary across regions that are large and differ greatly on dimensions other than institutions.

that on average increase household wealth should lead to aggregate growth. The household wealth variables are from Demographic and Health Surveys (DHS), which are large, nationally representative surveys, so the results are generalizable at least to the countries in the sample.[11] These surveys follow a standard form and have been carried out in many developing countries. They ask a wide variety of questions ranging from education and household characteristics to individual health and anthropometric information. However, DHS surveys do not measure income or wealth directly. They do measure the presence of goods in the household such as radio, television, electricity, etc. These variables are indicators of wealth, so I use wealth factor scores created from them.

There are several ways to create factor scores, and some DHS surveys include wealth factor scores derived by the principal components method. I prefer minimum distance techniques because they give greater weight to more precise indicators, fit a specific model, and provide a test of fit. I created wealth factor scores for the pooled data from all thirteen countries using minimum distance factor analysis.[12] These scores are the dependent variables in the regressions. At the household level I use the education, age, and gender of the household head as well as an indicator for urban areas as independent variables. I also created some regional controls from the DHS data. These include the mean education level in each region and a public goods factor score which was created from the regional mean levels of five goods (piped water, flush toilets, electricity, telephone, and dirt floors).[13] Table 3 presents summary statistics of the DHS data.

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[11] The countries are Ghana, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Nigeria, Senegal, South Africa, Tanzania, and Zambia.
[12] Please see Appendix 2 for a full description of the technique.
[13] This factor score was also created using minimum distance techniques as described in Appendix 2. The variables were too highly correlated to include in the regression together, thus factor analysis is used as a variable reduction technique here.

Table 3: Summary Statistics of DHS Variables

Variable	Mean	Std. Dev.	Min	Max
Wealth	0.00	1.00	-0.74	2.65
Incomplete Primary Ed.	0.27	0.44	0.00	1.00
Complete Primary Ed.	0.12	0.33	0.00	1.00
Incomplete Secondary Ed.	0.14	0.35	0.00	1.00
Complete Secondary Ed,	0.06	0.24	0.00	1.00
Higher Ed,	0.05	0.21	0.00	1.00
Age	45.52	16.04	9.00	98.00
Female	0.27	0.45	0.00	1.00
Urban	0.35	0.48	0.00	1.00
Public Goods Factor Score	0.00	1.00	-0.82	3.50
Mean years of Education	5.14	2.61	0.62	11.91

IV. Estimation and Results

I carried out weighted least squares estimation (the weights are the DHS survey weights) with wealth factor scores at the household level as the dependent variable. The household level independent variables are education dummies for level completed, age and age squared and dummies for female household head and urban area. The estimated coefficients on these household variables are significant and have the expected signs, which increases confidence in the wealth factor scores as a reasonable measure of wealth.[14] The regional variables are the six Afrobarometer regional institutional variables, two Afrobarometer controls,[15] the two DHS regional controls, and a regional level indicator for regions that are entirely urban (other regions are mostly rural but may be partly urban).[16] All estimations include dummies for the thirteen countries and adjust the standard errors for the regional clusters.[17]

[14] See the full regression reports in Appendix 4 for the coefficients on these variables
[15] These variables, Contact Local Rep and Contact National Rep, measure the percentage of people in a region who never contact their local or national representatives.
[16] Several specifications were tried with different formulations of the wealth factor scores and using Law-tax alone rather than Law. In general, the results are robust to

Table 4 presents the results. I began with the broadest possible model that includes each regional variable, its square and interactions with all other regional variables. Given this large set of closely related variables, multicollinearity could be a problem, so I first eliminated the interaction terms that had a correlation greater than .75 with any other variable. From this broad regression I then tested to see if a simpler specification was appropriate. I was able to clearly reject the joint test that all the interaction terms were zero and the joint test that all of the squared terms were zero, so the nonlinear specification is correct. While a linear specification would be easier to interpret, the results here indicate that a linear specification of the institutional variables would be inappropriate.

I next eliminated some of the institutional variables by considering each insignificant variable (at the 10% level) in turn and comparing a regression without that variable to one with it. If the coefficients on other variables changed when I dropped a variable, I kept that variable in the final specification so as to not introduce bias. Some of the variables were dropped, so the final specification does not include all of the interactions and squared terms in the initial regression, but it is still highly nonlinear.

Since the appropriate specification is nonlinear, I cannot present simple linear coefficients to test hypotheses. Instead, I follow the standard practice for nonlinear specifications of reporting the marginal effects of each variable at the means of any interacted variables, with one exception. The urban variable is an indicator for regions

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variations in how the factor scores were created. The Law-tax variable was jointly significant only at the ten percent level, but Law was significant at one percent, which suggests that more than just the perception of tax enforcement is important for wealth.

[17] To test the robustness I also computed standard errors under homoskedasticty, general heteroskedasticity (White estimator) and clusters at the country level. The p-values in table 3 only change marginally with these different calculations and all variables remain significant.

that are entirely urban, so it does not make sense to report a marginal effect for the average region that is partially urban when in reality the regions are either completely urban or almost entirely rural. The results presented in table 3 are therefore the marginal effects of each variable and its interactions calculated at the regional means for urban and non-urban regions. To test for the significance of a particular variable I then present the p-value of the joint test of the variable and all of its interactions.

Table 4: Marginal Effects of Institutional Variables

Variable	Marginal Effect at Means			Hypothesis	Confirmed?
	Non Urban	Urban	Joint p-value		
Crime	**-0.161**	**-0.310**	0.00	negative	yes
Law	**0.004**	**0.067**	0.01	positive	yes
Trust Army	**0.233**	**-0.278**	0.00	positive	no
Trust Local Rep	**-0.312**	**-1.182**	0.00	positive	no
Reps Corrupt	**0.047**	**-1.403**	0.00	negative	yes
Police Corrupt	**0.504**	**1.791**	0.00	negative	no

Regressions by weighted least squares with 118,262 observations and 151 regions. Wealth factor scores are the dependent variable; they are standardized so the units are standard deviations.

V. Discussion

Hypothesis 1 says that living in a region where fear of crime is higher will decrease household wealth. This hypothesis is supported by the data. The effect of the Crime variable is negative at the regional means and significant at the one percent level. Crime has a stronger negative effect on wealth in urban regions. In non-urban regions an increase of the Crime variable by one standard deviation is estimated to decrease wealth by 0.03 standard deviations on average; in urban regions a standard deviation increase in Crime on average reduces wealth by 0.05 standard deviations. The effect of Crime also interacts positively with Law. Figure one plots the marginal effect of Crime as Law varies across its range, holding all other variables at their means. It shows clearly that fear of crime has a stronger deleterious effect on wealth

when expectations of law enforcement are lower. This interaction suggests that improving the legal system will lessen the negative effect of crime on wealth.

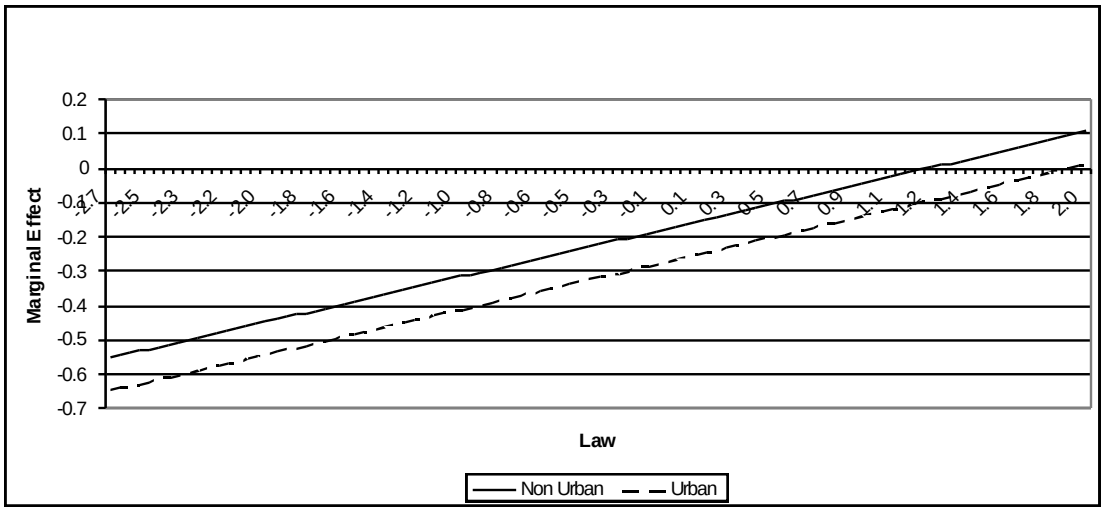

Figure 1: Marginal Effect of Crime with Law

Hypothesis 2 says that living in a region with a better legal system will increase wealth. The effect of Law is significant, but the marginal effect is near zero at the regional means in non-urban regions. This result is due in part to calculating the marginal effect at the means; the coefficients on Law also are smaller in magnitude because law is in units of standard deviations. A one standard deviation improvement in Law thus increases wealth by 0.004 standard deviations in non-urban regions and 0.067 standard deviations in urban regions at the means of all other variables. Figure 2 plots the marginal effect of Law as Trust Army varies from zero to one (it is a percentage), holding all other variables at their means.

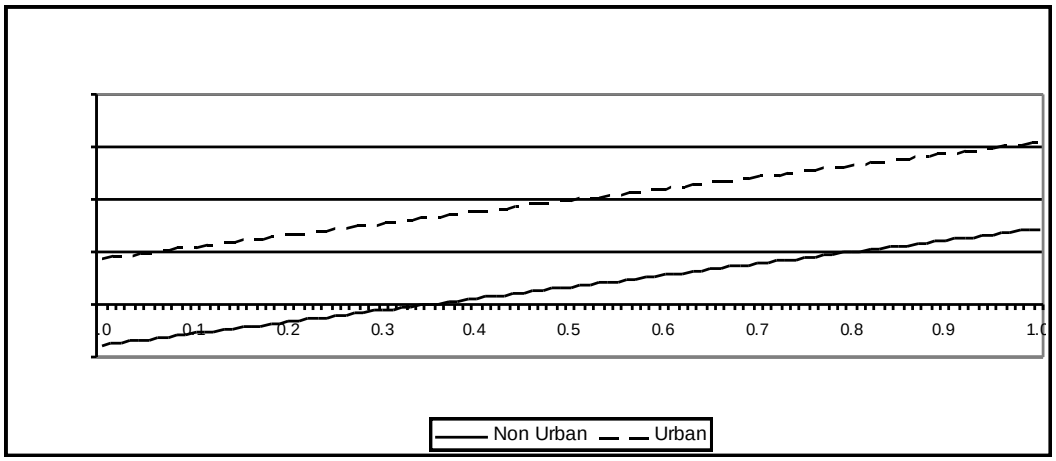

Figure 2: Marginal Effect of Law with Trust Army

It is clear in figure 2 that the effect of Law is stronger when Trust Army is higher for both urban and non-urban regions. Since Trust Army serves as a proxy for trust in national government, this intuitive interaction suggests that improving the legal system will be more beneficial for wealth when people have greater trust in national government. Another interesting result from figure two is that when trust is very low in non-urban regions the marginal effect of Law is negative, suggesting that having a stronger legal system imposes some costs on households.

Figure 3 shows the marginal effect of Law as Crime varies, holding all other variables at their means. This figure suggests that improving the legal system will have a stronger positive impact on wealth when fear of crime is higher. It also shows that if fear of crime is very low in non-urban areas then efforts to improve the legal system could be detrimental to wealth.

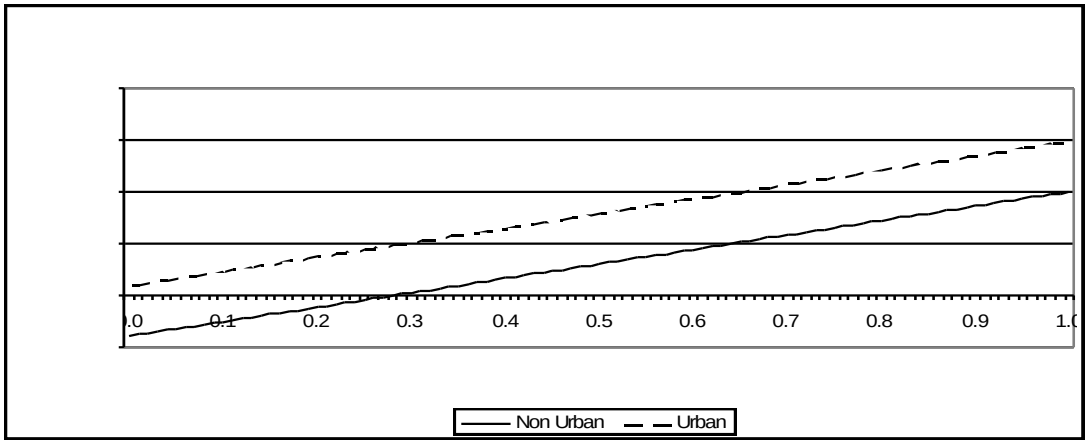

Figure 3: Marginal Effect of Law with Crime

Law thus has a clear positive effect in urban regions and a positive effect in non-urban regions when Crime and Trust Army are at or above their means. These results suggest that improving the legal system will be beneficial when people fear crime and trust national government more, but may backfire when crime is not a problem and trust is low.

It is surprising that the effect of Law on wealth is negative at any point. Law may capture to some degree the perception that the government will be oppressive, particularly when trust in the national government and fear of crime are low so government meddling is seen as threatening and unnecessary. It is plausible that African governments are prone to collect taxes in a heavy-handed fashion and higher expectations of law enforcement therefore mean lower expectations of freedom. Thus improving the legal system is associated with greater government interference. When people trust the government and fear crime this interference is beneficial, but if crime is not a problem and trust in the government is low government interference is detrimental to wealth in non-urban areas.

Hypothesis 3 says that living in a region with higher levels of trust will lead to greater wealth. The evidence here is surprising. The effect of Trust Army, which

reflects a general level of trust in national government (recall its high correlations with the other trust variables), is positive in non-urban regions but negative in urban regions at the means. The marginal effect of Trust Local Reps is negative. Given that both of these variables measure trust and that they are positively correlated (.64), it is surprising that they have the opposite sign. The significance of these variables suggests that trust is indeed important, but the negative signs suggest that perceptions of trust perhaps capture naivety rather than a good institutional setting. Alternatively, it is possible that higher trust in one's local representative reflects a greater reliance on local leaders, perhaps because they are at odds with the national government. Unfortunately, I lack data to investigate this further.

Hypothesis 4 says that living in a region with higher levels of corruption decreases household wealth. The evidence here is complex. Both corruption variables are significant, but their signs differ. In urban regions the marginal effect of Reps Corrupt is large and negative and the effect of Police Corrupt is larger and positive. In non-urban regions the effect of Reps Corrupt is positive but small, while the effect of Police Corrupt is also positive but not as large as in urban regions.

Let's focus first on urban regions. Theoretically, corruption can decrease wealth if it merely reflects wasteful government (the "cost effect"), but it can increase wealth if it allows individuals to bypass unwieldy red tape (the "grease effect") (Bardhan 1997, Paldam and Gundlach 2008). It is possible that we are seeing both effects here. The Reps Corrupt variable reflects the level of corruption in the government itself and in representatives whom individuals rarely come into contact with. It is not surprising then that in urban areas this variable captures the negative effects of worse governance, and the estimated marginal effect is large. A one standard deviation increase in Reps Corrupt is estimated to decrease wealth by 0.25 standard deviations in urban regions.

The Police Corrupt variable, however, measures the level of corruption in an arm of government with which individuals can expect to have contact. Thus if the government is heavy-handed and imposes strict regulations in urban areas, greater corruption among the police may indicate more possibilities for individuals to get around government rules. Thus corruption among the police is good and living in urban areas where the police are more corrupt increases individual wealth by a large magnitude: a one standard deviation increase in Police Corrupt is estimated to increase wealth by 0.3 standard deviations in urban areas. This line of thought is supported further by the negative interaction of Police Corrupt with Trust Local Rep shown in figure 4. This figure shows that when the level of trust of the local representative is low, the benefit of police corruption is larger.

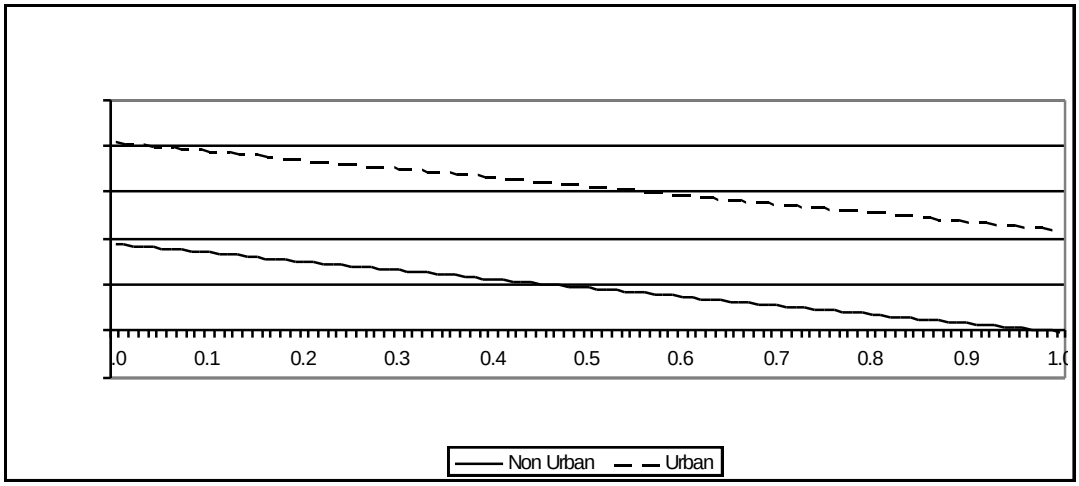

Figure 4: Marginal Effect of Police Corrupt with Trust Local Rep.

In non-urban regions the story is less clear. The effect of Reps Corrupt is small but positive here, perhaps because in places where the government is less present, this variable captures people's awareness of how government works. Thus the positive effect is due to greater awareness of the workings of government, and the deleterious effects of increased corruption in representatives are felt less in non-urban regions. Police Corrupt has a smaller positive effect in non-urban regions, so there are no negative effects of corruption at the means in non urban regions.

The multiple effects of corruption at different levels of government and the stark differences in urban versus non urban regions are an important result. Measures of corruption in the literature often come from surveys of businessmen who do business in the country. It is likely that these surveys have an urban bias because international business will naturally be focused in urban areas. Using these measures for the whole country then distorts the different effects of corruption in urban and non urban regions. The results here indicate that there are no negative effects of corruption in non urban regions, and that the "grease effect" may be slightly larger than the "cost effect" in urban regions (Paldam and Gundlach 2008). Policies aimed at reducing corruption may therefore have little or no benefit to household wealth.

VI. Conclusion

There are several interesting results from this analysis. The broad results are that the effects of institutions are non-linear and that within-country variation is important. Furthermore, the urban/non-urban and local/national dichotomies, as well as the different effects of corruption at different levels of government could not be captured by a single country-level variable. These results suggest that the country-level literature ignores important variation within countries and also may be underspecified because it cannot account for the nonlinear effects of institutions.

The specific results are that living in regions with greater fear of crime decreases household wealth and that living in urban regions where legal systems are better increases wealth. The positive interactions between the legal system and fear of crime and the legal system and trust are intuitive and interesting. These interactions suggest first that policies to improve the legal system will be better targeted at areas where fear of crime is higher but also where trust of national government is higher. The interactions also show that it is naïve to expect any of these variables to have an independent direct effect on wealth.

The first surprising result is the negative effect of improving the legal system when crime and trust in national government are at or below their means in non-urban regions, suggesting that higher expectations of laws being enforced can also mean a more oppressive government that imposes other costs and constraints on wealth.

The second surprising result is that there seem to be good and bad kinds of corruption, particularly in urban regions. Corruption in representatives, which proxies for the general level of corruption in the government, is detrimental to wealth in urban regions. However, corruption among the police has a positive effect on wealth in urban regions, suggesting that corruption at lower levels where people interact with the government may allow for more flexibility to bypass oppressive policies.

These results as a whole suggest that the workings of institutions are complex and that policies based on country-level data may be misguided. For example, the existence of positive effects of increased corruption, particularly among the police in urban areas, suggests that policy initiatives aimed at curtailing corruption of all types may fail to increase wealth. Policy makers need to be aware that there may be strong urban/non-urban and local/national dichotomies in institutional functioning in African countries. Policy initiatives will thus be better targeted where there is a clear effect, such as in reducing the fear of crime or improving legal systems in high-crime urban areas rather than attempting to weed out general corruption or promote democracy.

Finally, these results indicate that further research is needed. Dynamic effects of political institutions may be important but are not analyzed here. Future research will hopefully have panel data to investigate dynamic problems and will also develop clearer measures of regional institutions to further our understanding of the role these institutions play in growth and development.

Chapter 3

Are Institutions Fundamental for Development?

Evidence From Madagascar[18]

[18] This chapter was co-written with Christine Moser, Ph.D., Assistant Professor of Economics, Western Michigan University.

The literature on institutions and economic growth has shown that institutions are one of, if not the, primary factors in explaining cross-country differences in long-term development (Acemoglu and Johnson 2005, Paldam and Gundlach 2008, Pande and Udry 2005). However, the cross-country literature cannot "unbundle" institutions to illuminate the mechanisms driving these results and does not consider within country variation in institutions (Acemoglu and Johnson 20005). It is thus unclear if the results are driven narrowly by property rights institutions or broadly by many factors correlated with institutions. We analyze institutions and the development of the manufacturing sector at a very low administrative level in Madagascar, and we find that institutions are the key drivers of development of the manufacturing sector and the level of infrastructure. We see a strong narrow effect of formal land titles and find that institutions do vary in significant ways within Madagascar.

We use data from a countrywide commune census in Madagascar. Communes are the lowest administrative level in Madagascar, akin to counties in the U.S., and our census is a spatially explicit panel. We therefore have a larger sample and greater statistical controls than cross-country regressions. Specifically, we test and control for bias due to unobserved heterogeneity, spatial bias and autocorrelation and endogeneity. We find that endogeneity is by far the largest source of bias. When we account for the endogeneity of both institutions and infrastructure to development of the manufacturing sector, we find that land titling is the primary driver of both manufacturing and infrastructure. We interpret land titling as a proxy for a broader institutional environment, which we argue is fundamentally important for economic development.

II. Background

Acemoglu and Johnson (2005) claim that there is a "growing consensus among economists and political scientists" that institutions are "a primary determinant of

economic performance." (p. 950) This consensus comes from cross-country evidence that institutions have an important causal effect on long-term development (Acemoglu, Johnson and Robinson 2001, Rigobon and Rodrik 2005, Sokoloff and Engerman 2000) and that the effect of institutions is stronger than the effects of geography and trade integration (Rodrik, Subramanian and Trebbi 2004).

The cross country literature opens up many interesting questions. What are institutions, exactly, and how do they impact growth in such a significant way? While the literature has progressed steadily on developing robust cross country regressions, the mechanisms driving these empirical results are not discussed in great detail. The most common argument about how institutions drive economic growth is that property rights institutions protect investment, leading to better returns and lower risks for households and firms, an argument put forth by Acemoglu, Johnson and Robinson (2001) in their seminal paper.

The only further delineation of specific institutions in the cross country literature comes from Acemoglu and Johnson (2005), who make a broad distinction between property rights institutions and contracting institutions and show that property rights institutions are more important for long-term growth. However, Acemoglu and Johnson (2005) also stress that property rights variables at the country level represent a "bundle" of institutions whose individual effects cannot be distinguished.

At the country level, property rights institutions are measured by things such as the probability of expropriation by the government or constraints on the executive. The theoretical justification for these measures is the relatively simple idea that individuals and firms have greater incentives to invest when their property is less likely to be expropriated. Thus better property rights institutions are theorized to lead directly to better returns on investment.

This simple explanation belies the large explanatory power of institutions in cross-country regressions. We must consider that property rights may be capturing a broader institutional setting that is conducive to growth. Green (2008), for example, shows that regional (within country) variation in subjective security, law enforcement and corruption explains differences in household wealth in a sample of thirteen African countries. Green (2008) uses within-country variation in institutions and a multilevel identification strategy to test the effects of several institutions at once, which cannot be done in cross-country regressions. The limited number of instrumental variables available at the country level is one reason why the mechanisms of institutions have not been explored more thoroughly. Thus, while the cross-country literature has opened up many questions, we may need to turn to within-country data to answer them (Pande and Udry, 2005, Green 2008).

Micro and meso level research on institutions investigates property rights as well, albeit with different measurements. De Soto (2000) is famous for arguing for property rights at the household level in the form of formal land titles. He makes a similar theoretical argument to the one in the macro literature: better property rights allow for better returns on investment. Jacoby and Minten (2007) test this hypothesis directly in Madagascar by analyzing the investment decisions of households with and without formal titles on their land. However, they find no direct effect of owning a title on household investment. It is not clear then how differences in land titling at the micro level are related to the cross-country results.

Fafchamps and Moser (2003) as well as Fafchamps and Minten (2001) point out another aspect of property rights - the negative effect of crime on trade. Crime is costly not always through measurable theft, but through the costly adjustments made by individuals to avoid theft. Thus property needs to be protected from other individuals as well as from the government. This aspect of property rights is captured

by Green's (2008) measure of "fear of crime," which he finds is a significant determinant of household-level wealth. We can thus delineate two aspects of property rights at the micro level. The first is formal rights in the form of land titles and the second is informal protection from others in the form of subjective security.

Other research argues for the importance of infrastructure for growth. Pinstrup-Andersen and Shimokawa (2006) argue that infrastructure is systematically under-provided in African countries. Moser, Barrett and Minten (2005) explore how transactions costs due to poor infrastructure in Madagascar lead to poor price transmission and low market integration, and Kimenyi (2006) argues that public goods such as roads and schools have in many cases in Africa been distributed along ethnic lines. Moser (2008a) also finds evidence that public goods allocation is influenced by patronage and reelection concerns in Madagascar. Bates and Humphreys (2005) model provision of public goods by an elected government and argue that electoral accountability is key for public good provision in Africa. We must therefore consider the effects of infrastructure on economic development and consider the relationship between institutions and infrastructure. It seems reasonable to expect better institutions to correlate positively with better infrastructure, so it is possible that part of the country level effect of institutions is due to the positive effect of infrastructure.

In sum, we have a macro level literature that suggests that institutions are very important for development and ascribes their effects to a vague conceptualization of property rights. However, the small number of reasonable instruments for institutions at the country level severely limits further empirical investigation across countries. Micro and meso level work also suggests that property rights may be important, but considers subjective security, infrastructure and other institutions like corruption and trust as well. It is not clear then if the cross country results are driven by a narrow effect of property rights, as suggested by the macro literature, or by a broader

institutional setting that affects formal and informal property rights, infrastructure and other factors as well. The literature also cannot tell us if the effects of property rights in cross-country regressions should be present in within country regressions because the micro literature has mixed results on property rights alone.

In this paper, we ask the question: do institutions matter for economic development at a very low administrative level within a single country? We find that they do, and we further examine which institutions matter and the relationship between institutions and infrastructure in Madagascar. We find that formal property rights in the form of land titling are the key driver of employment in manufacturing and of the level of infrastructure. While our main effect is on the formal property rights variable, our entire results suggest that this variable may be a proxy for a better institutional environment. The next section describes the data. Section IV discusses estimation and Section V presents our results. Section VI concludes.

III. Data

This paper uses two rounds of a unique commune census from Madagascar. Communes are the lowest administrative unit in Madagascar, akin to counties in the United States. The first census was conducted in 2001 and the second in 2007. There were 1391 communes in 2001, but a new administrative structure split some large communes into parts so there were 1549 communes in 2007. Fortunately, we do have data on which communes were split so the periods can be matched. For this analysis, the 2001 communes are the base unit of analysis and the 2007 data was collapsed to the 2001 communes. In both periods, some communes could not be surveyed due to insecurity, so our base sample for both periods is 1375 communes.

Madagascar is divided into six provinces, 22 regions and 110 districts (fivondrana). In 2001, the regions existed in name but had no de facto administrative role (Moser 2008b). This situation changed with the new administrative structure put

in place in 2004, which emphasized regional government and shifted many administrative roles away from the provinces. Communes and fivondrana have been consistently present, but there have been some shifts in their administrative responsibilities as well (Moser 2008b). It is a strength of the data that we will be able to do analysis at different administrative levels; however, we must consider that communes, fivondrana, regions and provinces have different administrative roles in each period, which may lead to some differences in the estimations in each period or in a pooled sample.

The census collected detailed data on commune infrastructure, businesses, legal systems and agriculture from focus groups and administrative records in each commune. The focus groups were made up of residents from the commune, some of whom were randomly chosen and some of whom had administrative positions in the commune. While enumerators were instructed to interview a diverse group that would represent the whole commune as closely as possible, the data are not strictly representative of the communes, particularly their more remote villages. Some of the questions are subjective, which may introduce further noise. Thus there are some weaknesses to the data. However, to our knowledge the data is unique in being a panel countrywide census covering a wide range of variables at a low administrative level.

Our dependent variable is a proxy for the level of economic development (income data is not available at the commune level). The commune census asks whether small (<10 employees), medium (11-50 employees) or large (>50 employees) manufacturing / agri-business firms are present in the commune. When a firm is present, the census asks for an estimate of the number of employees. We combine the estimates of the number of employees of the first small, medium and large firms in each commune to form our outcome variable, Employment. Employment gives us a

continuous proxy for economic development. Unfortunately, the census does not record the number of employees for large firms if it exceeds 100. The Employment variable is therefore right-censored at 160 (10 for a small firm plus 50 for medium and 100 for large).

We are interested in policy initiatives for long term growth. To that end, we use variables measuring institutions, physical infrastructure and market infrastructure from the census. We will also make use of some geographic variables as exogenous controls. We first describe the institutional variables.

We distinguish here between two aspects of property rights: protection from other individuals, reflected in the crime / security situation, and protection from the government (local or national), reflected in formal legal rights. The commune census has variables that measure both of these aspects of property rights.

The first institutional variable, Security, is an indicator equal to one if the focus group rated the security situation as good or very good. This variable captures the group's subjective expectations about security, which we feel map well onto the degree to which they must make costly adjustments due to insecurity. There are objective indicators of security in the census (crime data, police presence, etc.), but we feel that the subjective indicator better captures people's expectations about security, which influence their economic decisions.[19]

The second institutional variable relates to land titling. The census asks the focus group to estimate the percentage of land in the commune that is titled, which indicates land that has a transferable title in the owner's name. The responses are categorical—from one to six—where one indicates zero percent, two indicates 1 – 5 percent, etc. Our variable Titled was computed from this data by counting the median

[19] We compared results using the objective indicators as well and they were not significantly different. See Appendix 5 for a summary of results with objective measures of crime.

value in the range for each number (so a number 1 was counted as 0 percent, number 2 as 3 percent, etc.). This variable is an admittedly noisy measure of land titling, but it does have the benefit of having a clear interpretation.[20] We thus have two measures of property rights institutions: Security and Titled.

The commune census also contains good data on the infrastructure in each commune. We divide these variables into physical infrastructure and market infrastructure. The physical infrastructure variables include the number of service stations in a commune and indicators for whether a commune has a paved road, an unpaved road, national electricity, cell phone service, landline phone service, community wells or public running water. The market infrastructure variables include the number of daily and seasonal markets operating in a commune and an indicator for whether formal credit is available in agriculture.

Lastly, we make use of geographic variables as exogenous controls. These include indicators for whether a commune borders the ocean, has a river, has a navigable river or has a forest. We also use a distance variable, DistanceUC, that measures the distance in kilometers to the nearest urban center, and Population, which is an estimate of the commune's population. Since population changes are likely endogenous to Employment, we use a two year lag of population. Table 5 presents the summary statistics of the commune variables.

[20] We also tried using indicators for each range of percentages and obtained very similar results.

Table 5: Summary Statistics

	Mean	Std. Dev	Min	Max
Employment	11.24	30.10	0	160
Institutional Variables				
Titled (%)	0.06	0.13	0	0.75
Security Indicator	0.73	0.44	0	1
Physical Infrastructure				
Paved Road	0.23	0.42	0	1
Unpaved Road	0.68	0.47	0	1
National Electricity	0.09	0.28	0	1
Landline	0.15	0.36	0	1
Cellphone	0.26	0.44	0	1
Community Wells	0.20	0.40	0	1
Running Water	0.12	0.33	0	1
Number of Service Stations	0.15	0.79	0	25
Market Infrastructure				
Number of Daily Markets	0.90	3.21	0	29
Number of Seasonal Markets	0.97	3.24	0	42
Formal Credit in Agriculture	0.23	0.42	0	1

We are interested in the effects of institutions and infrastructure on the development of the manufacturing sector. However, we first need to determine what the appropriate administrative level is for our analysis. For example, it is possible that road building is a district responsibility and therefore there is a wide disparity in roads between districts and little or no disparity in roads between regions. In terms of institutions, the implicit assumption of most quantitative research is that variation within countries does not matter, much less variation at the commune level. To shed some light on this question, we decompose the variance of seven key variables into the between commune, between district, between region and between province variation. Table 6 presents the decomposition.[21]

[21] Appendix 6 describes the decomposition of the variance in detail.

Table 6: Variance Decompositions

Variable	Between Commune Variance	Between District Variance	Between Region Variance	Between Province Variance
Employment	83%	12%	2%	3%
Titled	84%	11%	3%	1%
Security	86%	11%	5%	2%
Paved Road	88%	9%	2%	2%
Landline	72%	11%	5%	12%
Running Water	63%	20%	5%	12%
Number of Daily Markets	73%	21%	4%	3%

It is clear from the variance decomposition that most of the variance of all of our key variables is at the commune level. We will therefore focus our efforts on commune level regressions. We now turn to the estimation.

IV. Estimation Strategy

We want to estimate the effects of institutions, physical infrastructure and market infrastructure on the development of the manufacturing sector, which is measured here by the Employment variable. Our basic model is

$$y_{it} = \beta_1 * \text{Institutions} + \beta_2 * \text{Phys. Infra.} + \beta_3 * \text{Market Infra.} + u_{it} \quad (1)$$

The parameters of interest are the vectors β_1, β_2 and β_3. There are several potential problems that could bias least squares estimates of these coefficients and / or distort inference from our results. We need to account for potential bias due to unobserved time-invariant commune characteristics (i.e. fixed effects), endogeneity of both institutions and infrastructure, omission of spatial lags and censoring. We also need to correct our standard errors for the panel structure and spatial autocorrelation.

The first problem we will consider is unobserved commune heterogeneity. There may be unobserved, time-invariant characteristics of individual communes that correlate with the variables in the model and therefore bias estimates of the coefficients. Formally, this is the case if

$$u_{it} = c_i + \varepsilon_{it} \quad (2)$$

where c_i represents unobserved, time-invariant commune characteristics and ε_{it} is a mean zero stochastic error term. If c_i is correlated with any of the independent variables, estimates of β are biased. We will test for bias due to unobserved heterogeneity by comparing the estimates of β from the within estimator, which is equation (1) with fixed effects for each commune added in, with the between estimator, which is the regression using the commune means over time. If c_i is orthogonal to the independent variables, the estimated coefficients from the between and within estimations will be the same.

The second potential source of bias is endogeneity of both institutions and infrastructure. Endogeneity is the most common and most difficult problem in analysis of the effects of institutions on development. There are two ways to think about the endogeneity problem. The first is that there is some underlying process, like the choices of governments, that affects both the dependent and potentially independent variables. Because this process affects the dependent variable, it is in the error term. Because it affects the independent variables, the errors are then correlated with them and least squares estimates are biased.

The second way to think about endogeneity is reverse causality. For example, this would be the case if firms in communes lobbied successfully for better infrastructure, resulting in a high correlation in the data between infrastructure and Employment but no actual causation. Formally, this is to say that our X variable is really a Y and vice versa, and the end result is again biased estimates.

We account for the problem of endogeneity in several ways. The commune census includes some historical variables that can be used as instruments for institutions. The census recorded whether the French were present in a commune, whether the French had administrative facilities in a commune and whether other foreigners were present in a commune during the colonial period. Madagascar was

formally under French control from 1894 until 1960. It is reasonable to expect that communes where the French or other foreigners were present in the colonial period would develop more formal systems of property rights / land ownership to manage the competing claims of indigenous and foreign land users. We thus expect these variables to correlate positively with our measures of property rights, the Titled and Security variables. These colonial variables are valid instruments under the exclusion restriction that they do not have a direct effect on development of the manufacturing sector in the present, which seems realistic. Thus we account for the endogeneity of institutions with instrumental variables.

We also need to account for the endogeneity of infrastructure. We can instrument for some of the infrastructure variables with some geographic variables. Specifically, we have data on commune elevation and precipitation as well as the previously mentioned variables for the distance to an urban center and whether a commune borders the ocean. We can use these to instrument for cell phone service, public running water, the number of service stations and the number of daily markets. The necessary exclusion restriction is that these geographic variables must affect development of the manufacturing sector only through their affects on the aforementioned infrastructure variables. This exclusion restriction is questionable because there are other plausible pathways through which geography could affect manufacturing, which could lead to an overestimation of the effects of infrastructure when using these instruments.

Since the exclusion restriction for infrastructure instruments is weak we consider two other ways to account for the endogeneity of infrastructure. First we use a subsample of communes, those communes in 2007 that had no manufacturing firms in 2001. Since these communes had no manufacturing employment in 2001, it is unlikely that manufacturing firms there successfully lobbied for better infrastructure.

Using this subsample thus eliminates bias due to reverse causality. However, estimates of (1) may still be biased if there is an underlying process that affects both infrastructure and employment in manufacturing. We test for this second potential bias by again using Employment in 2007 in communes that had no employment in manufacturing in 2001, but regressing this 2007 Employment on the 2001 independent variables. If there is an underlying process that affects both Employment and infrastructure, then these estimates will be different from those in the same period. If infrastructure exogenously causes Employment, then we should still see an effect of infrastructure here, although it may be weaker due to the time lag. These subsample regressions give us two other ways to check the potential endogeneity of infrastructure since we lack strong instruments for the infrastructure variables.

To summarize, we account for the endogeneity of institutions by instrumenting for them with colonial history variables under a reasonable exclusion restriction. We account for the endogeneity of infrastructure in three ways. First, we instrument with geographical variables, although the exclusion restriction is admittedly weak. Second, we regress new Employment in 2007 on institutions and infrastructure to avoid reverse causality. Third, we regress new Employment in 2007 on institutions and infrastructure in 2001 to see if infrastructure and Employment arise contemporaneously or if infrastructure in the past causes new Employment in the present.

The third potential source of bias in least squares estimates of equation (1) is spatial bias. Since our units of analysis are contiguous communes, it is possible that outcomes in one commune may be affected by both outcomes and independent variables in neighboring communes. Formally, the spatial model is:

$$y_{it} = \alpha_1 y_{its} + \beta_1 * \text{Inst.} + \beta_2 * \text{Phys.Inf.} + \beta_3 * \text{Mar.Inf.} + \alpha_2 * \text{Inst.}_s + \alpha_3 * \text{Phys.Inf.}_s + \alpha_4 * \text{Mar.Inf.}_s + u_{it} \quad (3)$$

where the s subscript indicates a spatial lag, which is a weighted average of the values of that variable for neighboring communes. If (3) is the correct model (i.e. if the α's are not zero) then estimates of (1) may be biased due to the omission of spatial lags. The commune census identifies up to eight contiguous neighbors to each commune, allowing for the creation of spatial variables that equally weigh all contiguous communes. We can then estimate (3). We are interested in whether the α's are significant and in whether the β's differ from other specifications. Unfortunately, adding in spatial lags of endogenous variables may exacerbate bias due to endogeneity. Thus if the β's from (3) are significantly different from (1), we cannot identify whether the difference is due to spatial bias or to exacerbated endogeneity.

The last source of potential bias is the easiest to correct for. Since our dependent variable, Employment, is right censored, we use a Tobit estimator to account for bias due to censoring.

There are also some potential problems with inference. Specifically, the panel model of errors in (2) and the spatial model both suggest structure on the residuals that affects the calculation of standard errors. We thus consider corrections to the standard errors that account for the panel structure and for spatial autocorrelation. We run a random effects model that accounts for the structure in (2). The residuals in a spatial autocorrelation model have the following structure:

$$u_{it} = \lambda W u_{its} + \varepsilon_{it} \quad (4)$$

where λ is the coefficient representing the strength of the autocorrelation and W is a weighting matrix that assigns weights based on some measure of distance. We equally weigh all contiguous communes to each commune in the survey and give all other communes a weight of zero. We then estimate λ using minimum distance on the residuals and correct the standard errors.

V. Results

We first test for bias due to unobserved heterogeneity in equation (1). The test statistic comes from the comparison of the coefficients of the within and between estimators. The null hypothesis is that they are the same. We initially find chi^2 (17) = 23.07. The probability we would see a number this high under the null is 0.15, suggesting that there may be some bias. Upon closer examination, we find that several of the coefficients that differ greatly in the two estimations are highly insignificant in both, suggesting that we may just be seeing noise rather than strong bias. We therefore drop four insignificant variables and again test for bias due to unobserved heterogeneity. We now find chi^2 (13) = 14.51, which we would see under the null with probability 0.34. We conclude that some of the bias was simply noise and that there does not seem to be any strong bias due to unobserved heterogeneity.

Further examination of the within and between regressions shows that only three variables are really different with fixed effects.[22] Unpaved Road is negative and insignificant without fixed effects but positive and marginally significant with them. Number of Service Stations is positive and significant with or without fixed effects, but its coefficient doubles in magnitude with fixed effects, and the Number of Daily Markets is significant without fixed effects but insignificant with them. All other variables have nearly identical coefficients in the two regressions, suggesting that bias due to unobserved heterogeneity is small and is consigned to the three aforementioned variables. We thus conclude that commune fixed effects are not necessary.

We next consider potential bias due to endogeneity. We use instrumental variables; however, there is a question of what the appropriate sample is. Since we have shown that commune fixed effects are unimportant, we prefer to use the pooled sample from both periods because it is the largest. We are therefore treating the data

[22] See Appendix 7 for the results of the between and within estimations

as if we have 2750 individual communes rather than 1375 in two periods.[23] Using this pooled sample is only appropriate because commune fixed effects do not significantly bias the results.

Table 7 presents 5 regressions, all of which are variations of a random effects Tobit estimator on Employment. The first does not account for endogeneity of institutions or infrastructure in any way. It indicates strong associations between physical and market infrastructure and employment. Almost all of the infrastructure variables are strongly significant in it. It also indicates a significant positive association between formal land titling and employment. This first regression confirms that the things we are interested in understanding: employment, institutions and infrastructure, are closely related. The next four specifications attempt to sort out the causal relationships.

The second regression instruments for the two institutional variables, Titled and Security, in the pooled sample. It does not account for the possible endogeneity of infrastructure. The results here are informative. We find that Titled now has a much larger, nonlinear effect while many of the infrastructure variables are no longer significant and/or have smaller coefficients. These results indicate that institutions, specifically land titling, have a significant causal effect on Employment. The changes in the infrastructure variables from the first regression to the second suggest that many of the infrastructure variables are related to institutions as well and therefore the strong associations in the first regression were actually capturing part of the causal effect of institutions.

[23] We have also estimated the IV equations in the 2001, 2007 and between samples and the results are robust to the choice of sample. If anything, the effects of institutions are stronger in the smaller samples.

Table 7: Regression Results

Dependent Variable: Employment	Tobit: No Instruments	IV Tobit (instruments for institutions)	IV Tobit (instruments for institutions)	IV Tobit (instruments for institutions)	IV Tobit (instruments for institutions and infrastructure)
Sample	Pooled Sample	Pooled Sample	2007 communes with no manufacturing employment in 2001	2007 Employment, 2001 variables, communes with no Employment in 2001	Pooled Sample
Institutional Variables					
Titled	**43.53*** (11.96)**	-3.39 (60.75)	-151.78 (102.81)	72.73 (118.44)	**190.57* (105.92)**
Titled Squared	-13.00 (17.09)	**999.43*** (149.54)**	**1822.155*** (330.60)**	-7.34 (313.15)	
Security	0.08 (1.11)	10.26 (20.88)	-78.22** (35.08)	-40.67 (40.02)	30.32 (25.15)
Physical Infrastructure					
Paved Road	**8.16*** (1.18)**	**6.23*** (1.79)**	**6.45** (3.09)**	-0.83 (3.28)	
Unpaved Road	-0.80 (1.08)	-0.67 (1.37)	**7.21** (3.14)**	1.54 (2.28)	
National Electricity	**11.78*** (1.90)**	-0.37 (4.54)	0.07 (7.70)	-0.46 (8.88)	
Landline Phone Serv.	**12.64*** (1.75)**	**8.33*** (2.79)**	5.67 (4.43)	1.98 (6.73)	
Cell Phone Service	**3.21** (1.54)**	**3.05* (1.57)**	**4.83** (2.11)**	-1.05 (7.84)	-7.73 (28.62)
Community Wells	**4.58*** (1.26)**	3.14 (2.00)	3.18 (4.00)	-2.96 (3.41)	
Public Running Water	**14.36*** (2.12)**	**9.06*** (2.94)**	**9.62** (4.28)**		9.74 (22.92)
Number of Serv. Stat.	**6.26*** (1.14)**	**4.96*** (1.19)**	**7.59*** (1.73)**	-4.33 (4.56)	-6.45 (21.85)
Number of Serv. Stat.^{2}	**-0.23*** (0.07)**	**-0.18*** (0.07)**	**-0.28*** (0.08)**	0.45 (1.34)	
Market Infrastructure					
Number of Daily Mark.	**2.42*** (0.63)**	**1.71** (0.75)**	0.07 (1.10)	2.55 (2.03)	6.09 (5.48)
Number of Daily Mark.^{2}	**-0.07*** (0.03)**	-0.04 (0.03)	0.02 (0.05)	-0.26 (0.23)	
Number of Seas. Mark.	-0.60 (0.38)	**-0.74* (0.42)**	-0.19 (0.83)	0.39 (0.96)	
Number of Seas. Mark.^{2}	**0.03** (0.17)**	**0.05** (0.02)**	-0.02 (0.03)	-0.07 (0.07)	
Formal Credit in Ag.	0.18 (1.36)	0.97 (1.52)	**4.76** (2.32)**	**11.61*** (2.63)**	**18.18* (10.65)**
Observations	2740	2708	1066	1066	2332

The first two specifications indicate that institutions are indeed endogenous. They further suggest that institutions are closely related to infrastructure, which may drive some of the strong associations in the first regression. We now need to account for the possible endogeneity of infrastructure. As previously discussed, the bias due to endogeneity could potentially take two forms. The first is reverse causality, where employment actually causes infrastructure. We account for bias due to reverse causality by restricting our sample to communes in 2007 that had no manufacturing employment in 2001. In this sample, all of the positive values of Employment reflect new employment since 2001. It is very unlikely that new firms since 2001 could cause infrastructure by 2007, so this sample should avoid bias due to reverse causality.

The third column in Table 7 presents these results. This regression instruments for institutions in the same way as the second, and the results here are very similar to the second regression. We find a large positive (albeit nonlinear) effect of land titling and some positive effects of infrastructure, specifically cell phone service and the number of service stations. Two variables that were previously insignificant are significant in this specification: Unpaved Road and Formal Credit in Agriculture. This specification indicates that there is not much bias due to reverse causality. It also indicates that Formal Credit in Agriculture may be important for the development of new employment in manufacturing.

The second source of potential bias due to endogeneity is the presence of an unobserved variable, presumably representing the choice process of local government, that affects both Employment and infrastructure. Our third regression indicated that Employment does not seem to be causing infrastructure. However, it is possible that both Employment and infrastructure are caused by the government's choice process, which is unobserved. The implication is that both infrastructure and Employment would develop contemporaneously due to the choices of the local government, with

neither causing the other. We therefore want to test if infrastructure causes Employment or if an unobserved process seems to cause them both.

We first consider again the subsample of communes that did not have any employment in manufacturing in 2001, as we did in the third regression in Table 7. However, we now regress the 2007 employment data from this subsample on 2001 institution and infrastructure data. This regression gives us an indication of whether infrastructure causes Employment in the long-term or whether new employment and infrastructure develop contemporaneously. We find that only one infrastructure variable is significant: Formal Credit in Agriculture. All of the other infrastructure variables are insignificant and many of them change signs from previous specifications. Our institutional variables are insignificant here as well, but the sign and magnitude of the linear coefficient on Titled are consistent with other specifications. These results are consistent with the explanation that infrastructure and Employment develop contemporaneously, with neither causing the other.

Our results thus far suggest that infrastructure is endogenous and may have no causal effect on Employment. We test this again by instrumenting for some infrastructure variables. We instrument for Formal Credit in Agriculture with the same instruments we used for institutions, namely the presence or administrative capacity of the French or the presence of other foreigners during the colonial period. It stands to reason that communes where foreigners were present during the colonial period would develop more formal systems of credit. We feel that these instruments are reasonable for Formal Credit in Agriculture.[24]

We next instrument for four of the infrastructure variables that were significant in earlier specifications: Cell Phone, Public Running Water, Number of Service Stations and Number of Daily Markets. We use geographical variables as instruments:

[24] See Appendix 8 for first-stage results

whether or not a commune borders the ocean, the distance in kilometers to the nearest urban center, maximum elevation in the commune and mean precipitation in the commune. These instruments are admittedly not as strong as the historical variables for institutions. The weak exclusion restriction suggests that we may see positive effects from the instruments that are actually due to a direct link between the instruments and Employment.

The last column of table 7 presents these results. We find again that the only infrastructure variable that is significant is Formal Credit in Agriculture. These results are consistent with the results and explanation from the fourth regression, namely that infrastructure does not seem to have a causal effect on Employment. However, we do see a large significant effect from Titled here, suggesting that institutions do cause Employment. Thus, even under a weak exclusion restriction we see only one significant effect of infrastructure on Employment.

The regressions in table 7 suggest that both institutions and infrastructure are endogenous to Employment, and further that institutions cause employment while infrastructure does not. However, none of the regressions in table 7 account for spatial bias in the data. We next must consider whether spatial lags are appropriate in any specification and if their presence changes our assessment of the endogeneity. To test for spatial bias we introduce spatial lags of Employment and all of the dependent variables in each specification in table 7. We find that the spatial lags of all of the independent variables are insignificant and do not affect the coefficients of other variables in any meaningful way. The spatial lag of Employment, however, is positive and significant in most specifications, although it also has little effect on other coefficients. We conclude that there does not appear to be any large spatial bias and that our conclusions about the endogeneity of institutions and infrastructure are robust to spatial bias.

We now present our final regressions on employment in table 8. These regressions drop all of the infrastructure variables except for formal credit in agriculture, which we instrument for along with Titled and Security. The first specification does not include the spatial lag of Employment; the second does. We also drop the squared Titled variable as it is clearly insignificant in both regressions here. We find that both Titled and Formal Credit in Agriculture have significant positive effects on employment. These effects are tempered somewhat when we include the spatial lag of Employment, indicating a small degree of spatial bias.[25] The positive coefficient on the spatial lag indicates positive spillovers from Employment in neighboring communes.

Table 8: Final Regressions

Dependent Variable: Employment	IV Tobit	IV Tobit
Titled	**236.29*** (49.17)**	**182.26*** (46.48)**
Security	-27.48 (19.40)	-2.21 (18.36)
Formal Credit in Agriculture	**31.74*** (9.20)**	**18.27** (8.71)**
Spatial Lag of Employment		**0.50*** (0.03)**
Observations	2708	2706
Controls	Geographic Variables, District Dummies	Geographic Variables, District Dummies

Our results indicate that both institutions and infrastructure are endogenous to Employment. Specifically, the evidence here is consistent with the explanation that there is an underlying process that affects both infrastructure and Employment, and that this process is related to property rights institutions. To explore this further, we regress the infrastructure variables, with the exception of Formal Credit in Agriculture,

[25] Our test of spatial autocorrelation, described in part IV, indicates that it is not a problem for inference. In general, the standard errors we corrected for spatial autocorrelation are slightly smaller than the uncorrected ones. We also correct for possible panel structure of the errors with random effects, again without changing the significance of any variables of interest.

on our institutional variables, Formal Credit in Agriculture and the exogenous controls. We instrument for the institutional variables and Formal Credit in Agriculture. Table 9 presents the results. We find that Titled has a significant positive effect on six out of the nine infrastructure variables, suggesting that property rights institutions have a causal role in infrastructure.

This paper initially asked if institutions matter for development at low administrative levels. The answer in Madagascar is a resounding yes. Our results suggest that formal property rights play a causal role in both the development of manufacturing and in the level of infrastructure in Madagascar's communes. How do we interpret these results?

As discussed in part II, the macro literature on institutions offers a rather narrow explanation for the mechanism through which institutions affect growth: better property rights lead to better returns on investment (Acemoglu and Johnson, 2005). De Soto (2000) championed this argument at the micro level, but Jacoby and Minten (2007) find that households in Madagascar do not invest more in land with formal titles. It is possible, however, that firms will invest more in land with formal titles.

The narrow explanation for our results is that formal property rights encourage significantly more firm investment. Formal titles can help firms achieve better returns and / or lower risks on their land, leading to more employment in manufacturing. Madagascar has a history of expropriation of land during the Ratsiraka government in the 1970s, thus firms may justifiably feel the need for formal transferable titles to their land. This mechanism explains why Titled has a positive effect on Employment. However, it does not explain the results we see in infrastructure.

Table 9: Infrastructure Regressions

Dependent Variable:	Paved Road	Landline Phone Service	Cell Phone Service	National Electricity	Community Wells
Estimator	Probit	Probit	Probit	Probit	Probit
Titled	**6.43****	**11.59*****	**4.94***	**29.44*****	2.02
(std. err.)	**-2.54**	**-3.04**	**-2.61**	**3.62**	-2.65
Security	2.12	-0.27	-2.83	3.03	0.61
(std. err.)	-1.74	-1.96	-1.73	-2.36	-1.78
Formal Cred in Ag.	-0.22	0.48	0.68	0.22	0.59
(std. err.)	-0.80	-0.98	-0.78	-0.95	-0.78
Observations	2670	2594	2552	2374	

Dependent Variable:	Public Running Water	Number of Service Stations	Number of Daily Markets	Number of Seasonal Markets
Estimator	Probit	OLS	OLS	OLS
Titled	**7.88****	**2.65****	0.85	3.45
(std. err.)	**-3.22**	**-1.1**	-4.73	-5.38
Security	-0.27	0.66	-6.69	-0.67
(std. err.)	-2.02	-0.88	-3.82	-3.00
Formal Cred in Ag.	0.60	0.61	2.64	**3.21***
(std. err.)	-0.93	-0.44	-1.85	**-1.65**
Observations	2328	2708	2708	2714

In Table 7, we see that many of the infrastructure variables that are significant in the first regression are insignificant in the second after we instrument for institutions. We can now suggest that this change is due to the effect of Titled on infrastructure, as seen in table 8. However, several infrastructure variables remained significant in the second regression and did not lose their effect until we accounted for the endogeneity of infrastructure. This result suggests that there is something else, an unobserved process outside of formal property rights, that affects both Employment and infrastructure.

Our results indicate that formal property rights are related to this underlying process, which probably reflects local governance. Indeed, a broad interpretation of

the property rights results would suggest that they are capturing a good institutional environment rather than a direct effect of improved returns on investment. We find this interpretation plausible because it offers an explanation for both the strength of the coefficient on Titled and the effects we see on infrastructure.

This broad interpretation suggests that there are other related institutions that are important for development of the manufacturing sector and for infrastructure. Green (2008) finds significant effects of subjective security, expectations of law enforcement and corruption in a sample that includes Madagascar. We do not find significant effects of subjective or objective security here, and we also have some measures of how people perceive local courts which are never significant. However, expectations of law enforcement and corruption may be part of the process that we do not capture here.

We also find a significant estimated impact of formal credit in agriculture on employment in manufacturing, which is surprising at first glance. However, there are several reasons why credit in agriculture might matter for manufacturing. It can increase fungibility for entrepreneurs operating agriculture and non-agriculture enterprises, there may be forward and backward linkages between small farmers, agribusiness and manufacturing, especially for cash crops that require post harvest processing.

VI. Conclusion

This paper analyzes the role of institutions and infrastructure in the development of the manufacturing sector at the commune level in Madagascar. We find that institutions do matter at such a low administrative level. Formal land titles have a significant positive effect on employment in manufacturing that is robust to bias due to unobserved heterogeneity, endogeneity, and omission of spatial lags. Furthermore, formal property rights have a causal role in the development of

infrastructure. We interpret the coefficients on our property rights variables as indications of an institutional environment that is conducive to economic growth rather than as a narrow effect of better returns on investment. While we do have strong evidence that institutions matter at very low administrative levels, further work needs to be done to shed light on the mechanisms through which institutions affect development.

Chapter 4

Democracy and Institutions in Postcolonial Africa

"If liberty and equality, as is thought by some, are chiefly to be found in democracy, they will be best attained when all persons alike share in the government to the utmost." – Aristotle, *Politics*

While Africa lags behind the rest of the world in economic development, the continent as a whole has experienced a wave of democratization since the end of the Cold War. More African countries are democratic and fewer are autocratic today than ever before. This growth of democracy raises some interesting questions for Africa: does it mean that African countries are "getting institutions right" and will achieve steady economic growth (Rodrik 2004)? Is it improving conditions for people in the more democratic countries? Does it reduce the likelihood of civil war, which has been so destructive in Africa?

This paper carefully considers the available evidence to answer these questions through cross country analysis. I analyze the data with two distinct methods to account for bias due to endogeneity while also considering country and time fixed effects. I find that when country and time fixed effects are included in instrumental variables regressions the significant effects of democracy vanish. However, simultaneous equations results, which fully account for all endogenous variables and still include time and country fixed effects, indicate that more democratic countries will achieve much stronger economic growth and have lower death rates.

Section II illustrates the background of democracy and institutions in postcolonial Africa. Section III focuses on methodology and the problem of identification in cross-country regressions and Section IV presents instrumental variables (IV) regression results. Section V describes the minimum distance simultaneous equations approach and presents the results of those estimations. Section VI concludes.

II. Democracy in Postcolonial Africa

Since the end of the Cold War, Africa has become steadily more democratic. This trend is easily seen using Polity scores, which are perhaps the most common measure of democracy in academic work.[26] Figure 5 shows the trend of the mean Polity score for Africa, where the sample includes all continental Africa countries; each country enters the sample at independence (or for the few that were independent before the data series begin, when data become available). The Polity score is calculated by taking a country's democracy score, which ranges from zero to ten, and subtracting it's autocracy score, which also ranges from zero to ten (Marshall and Jaggers, 2007). The resulting measure thus ranges from negative ten to ten. The trend is clear: the mean score hits a trough in the late 1980s and then begins its ascent around 1990. It briefly decreases in the late 1990s and then continues to rise.

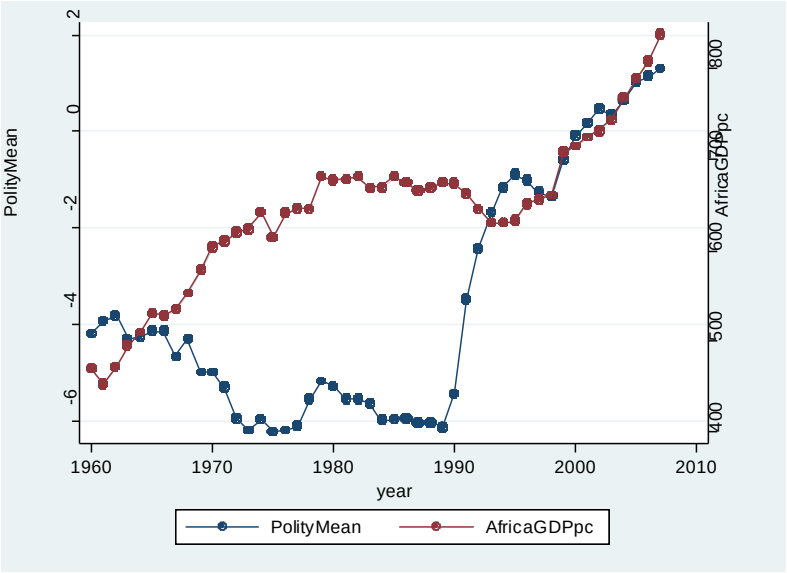

Figure 5:Mean Democracy Score and GDP per capita for Africa

Sources: WDI and POLITY IV

Figure 5 illustrates a wave of democratization in Africa starting in 1990. Research on democratization suggests that such waves are common throughout

[26] The POLITY dataset is publicly available at www.systemicpeace.org.

history; Europe experienced several waves in the nineteenth century and they have been the subject of close study (Huntington 1996, Van de Walle 1999). These waves occur in part because countries do not exist in isolation. When one country starts a revolutionary shift towards democracy, people in neighboring countries take notice and become more likely to attempt a change in their country, particularly if their neighbor is successful.[27] A similar mechanism was at play in the independence movements of African countries, which also came in a large wave beginning in the late 1950s. Naturally, people tend to regard such a wave of democratization as a good thing, but it is important to consider analytically whether it might be good or possibly bad.

Scholars argue that democracy is good for several reasons. It is associated with greater freedom, which is intrinsically good. Democracy is also associated with better governance, which can mean better institutions that will enable economic growth (Acemoglu, Johnson and Robinson 2001) and better public goods that will improve welfare. Lastly, democratic countries are generally considered to be more stable and less prone to war. The wave of democratization in Africa is clearly good in terms of expanded freedoms for people living in more democratic countries. I examine further if these more democratic countries have an advantage in economic growth, in the welfare of their people and in peace and stability.

There tends to be a strong cross-country correlation between democracy and GDP per capita for the world as a whole (Acemoglu et al. 2008). The relationship between democracy and national income has thus been studied carefully. Some argue that economic growth and the expansion of a middle class leads to more democratic government (Paldam and Gundlach 2008), while others claim that good institutions,

[27] Charismatic revolutionaries may spread such a wave as well. A fascinating historical example is Giuseppe Garibaldi, the Italian revolutionary who led liberal insurgencies in Italy several times, Brazil, Uruguay and France (Coppa, 2004).

which are associated with democracy, are necessary for economic growth to happen (Acemoglu et al. 2001, Rodrik et al. 2004).

Figure 5 also plots the per capita GDP for all of Africa; it shows three trends. Continent-wide GDP grew from the independence period of the early 1960s until around 1980. From there it decreased until the early 1990s, at which point it began an ongoing steady climb. The question is what is the relationship between GDP and democracy for Africa as a whole?

Before 1990 there is no discernable positive relationship between GDP growth and democracy. If anything, African countries on average became less democratic from the early 1960s to 1980 while their economies grew. Both variables are relatively stagnant during the 1980s, then the mean Polity score begins its increase five years before GDP starts growing again in the mid 1990s. For the rest of the sample both are increasing, suggesting that the wave of democratization may have contributed to the later turnaround in GDP growth. There is no indication here that higher GDP contributed to the overall rise in democracy.

Those who argue that democracy is good for growth tend to focus on institutions. Specifically, Acemoglu and Johnson (2005) use Polity's Executive Constraints variable as a measure of property rights institutions, which they argue are critical for growth. The Executive Constraints variable scores a country from one to seven on how many formal constraints exist to executive power. A higher score indicates greater constraints, which is interpreted as being indicative of better property rights (Acemoglu and Johnson, 2005). If the wave of democratization contributed to subsequent economic growth in Africa, it is possible that the mechanism was improved property rights. Figure 6 plots the mean value of the Executive Constraint variables for Africa against GDP per capita. The result is very similar to Figure 5. The mean of the Executive Constraints variable begins to increase about five years

before GDP growth resumes again in the mid 1990s. It is therefore possible that improved institutions contributed to the growth that began in the mid-1990s; however, there is no positive trend between Executive Constraints and GDP per capita prior to 1990. For the whole sample, the correlation between a five year lag of Executive Constraints and per capita GDP is small but positive at .07, although it is statistically significant at the one percent level. The correlation of a five year lag of Executive Constraints and growth is .08, also significant at the one percent level. It is noteworthy as well that the correlation of a five year lag of Polity with GDP per capita is only .02 and is not significant, indicating that the focus on Executive Constraints may be appropriate.

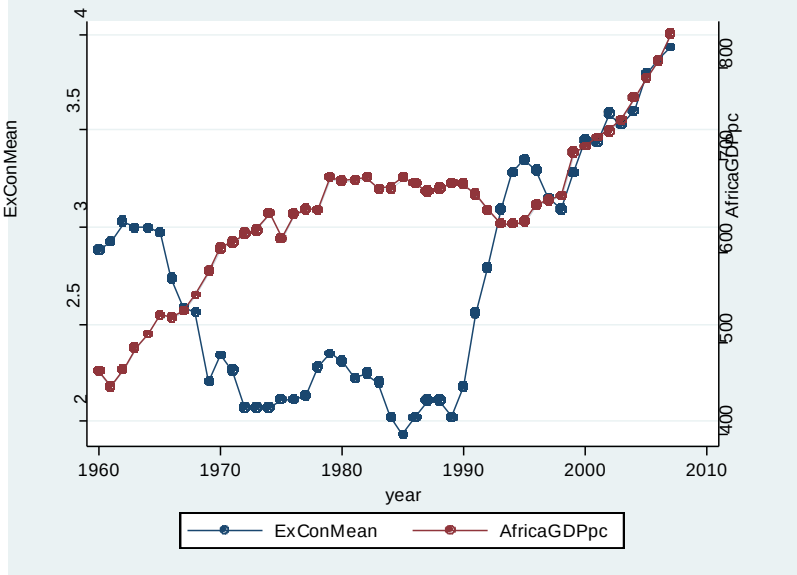

Figure 6: Mean Executive Constraint Score and GDP per capita for Africa
Sources: WDI and POLITY IV

Democratic government may produce better public goods, such as hospitals, that improve welfare even without leading directly to economic growth. For example, Sen (1999) argues that after India became independent and democratic famines there were much less devastating because the government organized a response and prevented mass casualties.

Unfortunately, welfare indicators such as mortality rates, education data or nutritional data are not widely available for African countries for the whole postcolonial period. The best indicator that is available for the whole period is the crude death rate, per thousand population, from the WDI database.[28] Figure 7 plots the mean death rate for all of Africa. The death rate has shown a steady decline for the entire postcolonial period. While this overall trend is not obviously related to the wave of democratization, it is possible that the death rate has decreased more in more democratic countries. The correlation between death rate and Polity scores for the whole sample is -0.11, significant at the one percent level.

It should be expected that HIV / AIDS has had a negative impact on the crude death rate in many African countries. However, the impact of HIV / AIDS in a given country directly depends on the policy response to the virus, thus amplifying any potential relationship between democracy and the death rate. Even for countries unaffected by HIV / AIDS, the crude death rate remains an intrinsically valuable variable and a reasonable proxy for well-being.

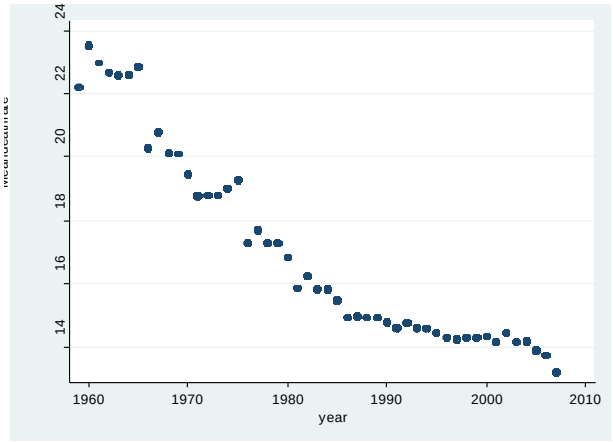

Figure 7: Mean Death Rate for Africa

Source: WDI

[28] Details about the World Development Indicators (WDI) database can be found at www.worldbank.org

It has been argued that democratic governments are less prone to wars internal and external. The logic is that democratic governments are more inclusive, which allows internal opposition groups to voice their concerns through the political system rather than turning to violence, obviating civil conflicts. Although interstate wars have been rare, civil wars have unfortunately been all too common in postcolonial Africa. Figure 8 plots the number of civil wars taking place by year in postcolonial Africa. The data for civil violence comes from the Major Episodes of Political Violence (MEPV) dataset from the Center for Systemic Peace.[29] I use their Civil Violence variable, which puts total civil violence on a scale from one to ten, where one represents sporadic political violence and ten represents "extensive, systematic and indiscriminate destruction of human resources and / or physical infrastructure" (Marshall, 2006). A three for this variable represents serious political violence, while a four marks the jump to "serious warfare" (Marshall, 2006). For Figure 8, any episode of violence rated four or higher is counted as a civil war.

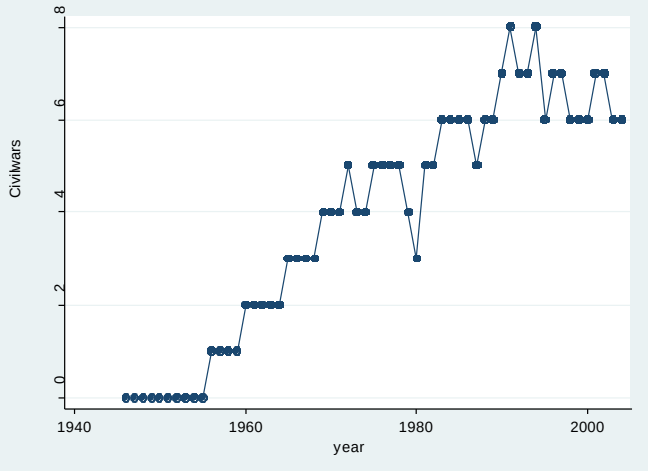

Figure 8: Civil Wars in Postcolonial Africa, Number of Episodes Per Year
Source: MEPV dataset, Center for Systemic Peace

[29] The dataset is available at www.systemicpeace.org

The unfortunate trend is that the number of ongoing civil wars has gradually increased in the postcolonial period. The wave of democratization does not seem to have decreased the overall number of civil wars. However, what the wave shows is a trend of political change across the continent, so it is not surprising that such a massive change includes many episodes of civil conflict. It is still worth investigating whether the countries that have opened their political process have decreased episodes of civil violence. To examine this further, I turn to Polity data on political participation.

The Polity project defines ten participation concepts based on country scores for regulation and competition in the political system (Marshall and Jaggers, 2007). Countries are counted as Dictatorships if they score a one or a two, which indicates "repressed competition," where one person or party dominates the political process and excludes others from participating. Factional states score a six or a seven, which indicates restricted competition based on competing political factions, without any one being dominant.[30] Finally, Democratic countries here are those that score an eight, nine or ten, indicating that the political process is competitive and liberalized or transitioning to a competitive liberal regime. Table 10 presents the type of participation regimes, as defined by Polity, in African countries by decade.

Table 10: Political Participation by Decade – Number of Countries of Each Regime

Participation Regimes in Africa	1965	1975	1985	1995	2005
Dictatorship (repressed)	27	42	40	17	5
Factional (restricted)	5	0	0	13	19
Democratic (liberalizing)	1	3	4	10	14

Source: POLITY IV

[30] A participation score of 3,4, or 5 indicates a transition from Dictatorship to an "uninstitutionalized state" and then a transition to a Factional state (Marshall and Jaggers 2006). Very few countries in the sample fell into these categories.

The trend in Table 10 is clear and consistent with the wave of democratization in Figure 5. Most African countries were dictatorships through the late 1980s, at which point many began to shift towards more competitive participation regimes. By 2005 the number of dictatorships had dropped to five from 40 in 1985, while the number of democratic / liberalizing countries was up to 14 from four in 1985. The number of factional regimes also increased drastically to 19 in 2005 from zero in 1985. Since the end of the Cold War there has been a major trend towards democratic governments with greater political participation in Africa. How does this trend relate to civil violence?

Table 11 presents summary statistics for the Civil Violence variable by lagged participation regime. It shows a clear trend. Factional regimes are the most prone to civil violence, followed closely by dictatorships, while democratic / liberalizing regimes are much less prone to civil violence. Indeed, the mean civil violence scores for dictatorships and factional regimes are close to one, indicating that in an average year those regimes experience sporadic political violence. Taken together, Tables 10 and 11 indicate that the continued episodes of civil violence in Africa may be associated with transitions from dictatorships to factional regimes and from factional regimes to democratic regimes. One can hope that the wave of democratization will continue and episodes of civil war will decrease as more countries open their political systems.

Table 11: Civil Violence by Participation Regime

Participation Regime	Civil Violence Mean	Std. Dev.
Dictatorship (repressed)	0.74	1.73
Factional (restricted)	0.91	1.78
Democratic (liberalizing)	0.10	0.39

Source: POLITY IV and MEPV dataset

The data presented in this section indicate that Africa is experiencing a wave of democratization that began at the end of the Cold War. This wave has included an

improvement in the mean level of Executive Constraints, which scholars have argued is critical for economic growth (Acemgolu and Johnson, 2005). Death rates have consistently decreased throughout the period; it is possible that continued democratization can further reduce death rates across the continent. Finally, the data indicate a massive shift in political participation from dictatorships into to factional and democratic regimes. Dictatorships and factional regimes are more prone to civil wars, which remain common in Africa. Hopefully the number of civil wars will drop as more African countries become democratic. The next step is to see if the loose associations identified here hold up in econometric analysis.

III. Identification in Cross Country Regressions

There is a long literature of cross country regressions looking at institutions and GDP levels or growth, which can be broadly divided into three phases based on the question of identification. The initial phase began when reasonable cross country data first became widely available. Data sets typically had small samples that limited the number of variables in any one regression, so the first identification concern was simply a question of which variables belonged in a small sample regression. Many papers presented different variables that were significant in some specification of a cross-country regression on growth or GDP. This first phase culminated in Sala-I-Martin's (1997) "million regressions" paper, in which he attempted to settle the question of which variables belong in cross country regressions by actually running several million regressions and seeing which variables were consistently significant. His results indicate that political institutions are significant determinants of GDP; however, his million regressions were OLS regressions that did not account for endogeneity.

Acemoglu, Johnson and Robinson (2001) began the second phase by finding plausible instruments for institutions in a cross-country regression. Their seminal

paper has spawned many other works using instrumental variables to identify the effects of endogenous variables on growth or GDP. Rodrik, Subramanian and Trebbi (2004), for example, use instruments for institutions and trade integration to test the effects of institutions, trade integration and geography on GDP across countries. They find that institutions "rule" over trade integration and geography as a determinant of GDP. Miguel et al. (2004) use rainfall as an instrument to show that negative growth shocks increase the likelihood of civil war in Africa. Acemoglu and Johnson (2005) attempt to push the IV approach as far as possible and conclude that due to the paucity of instruments it is impossible to "unbundle" institutions at the country level. Although Acemoglu and Johnson (2005) conclude that country level institutions cannot be "unbundled," the IV literature formed a consensus that institutions are a significant determinant of long-term growth across countries. Research in this phase focused on parsimonious specifications, being careful to not include variables that might be endogenous without proper instruments.

This second phase is now coming to an end, interestingly, due to the paper "Income and Democracy" by Acemoglu, Johnson, Robinson and Yared (2008). In "Income and Democracy," they put together a panel of countries and show that when country and time fixed effects are introduced, the effect of income on democracy disappears. Their paper focuses solely on the effect of income on democracy in the presence of fixed effects; they do not argue that it calls into question the earlier IV results. However, the instruments used in most of the IV regressions are time-invariant, so they cannot be used in a specification that also includes fixed effects. It is therefore an open, and very important, question as to whether the IV results hold with country and time fixed effects.

The focus on endogeneity in the IV literature led to intentional omission of variables. When fixed effects are introduced in a small sample, they capture a large

portion of the variance so it is to be expected that coefficients change drastically. The variance in a panel of countries can be divided into the within and between variance, where the within variance is measured as variation from a country specific mean and the between variance is variation between the country specific mean and the pooled mean. Country fixed effects effectively capture all of the between variance, leaving only the within variance to identify the coefficients of interest. The within variance makes up 77% of the variance of Growth, but only 34% and 40% of the variance of Death Rate and Civil Violence respectively. Of the institutional variables, the within variance makes up 48%, 47% and 60% of the variance of the Polity, Executive Constraints and Participation variables. I thus expect fixed effects to markedly change the results of IV regressions on institutions.

Since IV specifications intentionally left out many endogenous variables, the types of IV regressions seen in the second phase of this literature are now called into question and must be seen as underspecified. Thus the literature has now entered phase three, where it is necessary to account for country and time fixed effects.

Accounting for the endogeneity of institutions while also including country and time fixed effects requires an instrumental variable that varies across countries and over time. The common instruments for institutions in the literature are settler mortality rates and indicator variables for different colonial masters, neither of which varies over time. The literature as it stands is thus in a quandary: researchers can use the accepted instruments to account for endogeneity or include fixed effects, but not both.

I present two estimation methods here that account for endogeneity and include fixed effects. The first introduces an instrument that varies across countries and over time; I claim that a weighted average of Polity scores of neighboring countries is a plausible instrument for democracy and I present such IV estimates in the following

section. The second method uses a minimum distance simultaneous equations estimator to account for all endogenous variables while still including fixed effects. I outline this method fully and present the results in Section V.

IV. Instrumental Variables Estimates with Fixed Effects

In section II I outlined the trend of democratization in Africa and presented three mechanisms through which democracy may be instrumentally good. The first is that democracy may lead to better institutions, which then promote growth. I investigate this trend in a regression of Growth on the Executive Constraints variable. The second is that democracy may improve welfare, which is measured here by the Death Rate, which I regress on the Polity score. Finally, democracy may obviate civil war through a more inclusive politics. I investigate this by regressing the Civil Violence variable on political participation. For all three regressions, I use a spatial lag of Polity scores as the key instrumental variable.

A spatial lag is a weighted average of the values of a variable for neighboring countries. The intuition behind spatial lags is that in a sample of contiguous units it is likely that the effects of some variables will be present in neighboring units. The types of weights used to calculate spatial lags vary; for this research I use inverse distance from the country center as the weight because it is clearly exogenous and it gives close neighbors a stronger weight than those further away. The spatial lag of Polity thus gives a value for each country in each period that is the weighted average of the Polity scores of all other countries in the sample, with the closest neighbors receiving the strongest weight.

A valid instrument must meet the exclusion restriction that it correlates strongly with the endogenous covariate but does not affect the dependent variable directly. For the spatial lag of Polity to be valid it must correlate strongly with the endogenous covariates: Executive Constraints, Polity and Participation, but have no

direct effect on Growth, Death Rate or Civil Violence. I claim first that it is reasonable to assume that the Polity scores of other countries do not have a direct effect on Growth, Death Rate or Civil Violence in any one country simply because policies affecting these variables are made by countries, not by their neighbors.

I claim next that the level of democracy in neighboring countries should affect the level of democracy in a given country, thus leading to a positive correlation between Polity scores and the Polity spatial lag. I outlined earlier the trends in democracy in postcolonial Africa and the wave of democratization that began around 1990. The fact that such a wave exists suggests that democratization in one country is associated with democratization in another; indeed, history is full of such incidences. The timing of the wave is no coincidence either. The end of the Cold War was seen as the victory of the democratic / capitalist system and the wave of revolution in Eastern Europe clearly was felt in Africa as well. Thus the history of democracy is full of horizontal expansion – across many countries at the same time rather than independently in each country. It is reasonable then to expect the spatial lag of democracy to correlate strongly with the level of democracy in any country. Indeed, the correlations with the variables of interest are positive: the spatial lag of Polity has pairwise correlations of 0.33 with Executive Constraints, 0.24 with Polity and 0.20 with Participation.

The other estimation concerns are including country and time fixed effects and including other endogenous variables. I present four regressions for each dependent variable. The first two do not include other endogenous covariates. The first and third include time and country fixed effects while the second and fourth do not. These regressions give an indication of whether or not fixed effects change the results and whether adding other endogenous covariates changes the results.

The data used here have been pulled together from many sources, which are detailed in the descriptive tables and in the references. The base sample is the continent of Africa. I limit the sample to Africa because it is more fitting to a regression framework than other parts of the world for two reasons. First, the borders of African countries were largely determined by the colonial powers in Berlin in 1884-85, so they are clearly exogenous to the current political regimes. Having exogenous borders makes cross country regressions more believable because the borders delineate the units of analysis. In places like Europe or Southeast Asia where borders were determined by political and military power it is possible that the delineation of the units of analysis is endogenous to the variables of interest.

Second, African countries became independent relatively recently, mostly within the same time period, and they have had similar experiences both prior to and after independence. The independence date thus gives a clear start point for each country to enter the data and the similarity of experiences makes comparisons across countries more believable. The relatively recent event of independence from a colonial regime also provides for a set of time-invariant exogenous variables measuring conditions at independence.

The next three tables present descriptive statistics of the variables used. The data are an unbalanced panel that has been averaged over three year periods. Data are not available for all African countries in every period, but I have attempted to find the most expansive reliable data to cover as much of the postcolonial period as possible. The final sample has 411 observations over 13 periods covering the years 1963 to 2001. It includes 45 countries, nearly every country in Africa.[31]

[31] The sample does not include the small island states of Cape Verde, Comoros, Sao Tome and Principe, Seychelles and Mauritius. The only mainland countries not in the sample are Equatorial Guinea, Somalia, Liberia and Western Sahara, each of which lacked sufficient data.

Table 12: Summary Statistics for Endogenous Variables

Variable	Source	Mean	Std. Dev.	Min	Max
Growth of GDP per capita	WDI	0.71	3.87	-12.62	17.38
Death Rate (per thousand)	WDI	15.84	4.99	4.03	37.62
Civil Violence (sum over period)	MEPV	1.83	4.41	0.00	21.00
Savings (% of GDP)	WDI	12.60	10.63	-24.53	46.72
Trade (% of GDP, lagged one period)	WDI	62.49	31.97	9.31	171.00
Aid per capita (US$, lagged one period)	WDI	33.95	31.87	0.59	239.91
GDP per capita (US$, lagged one period)	WDI	693.59	960.53	94.88	7456.51
Executive Constraints (lagged one period)	POLITY IV	2.39	1.46	1.00	7.00
Polity (lagged one period)	POLITY IV	-4.73	4.83	-10.00	9.00
Participation (lagged one period)	POLITY IV	2.89	2.84	1.00	10.00

Table 12 presents the summary statistics for ten endogenous variables. These variables are endogenous because they are all affected directly and indirectly by the choices of the government. Growth and Death Rate are averaged over each three year period while the Civil Violence variable is summed over each period. Savings is national savings as a percentage of GDP at the beginning of the period. Trade, Aid per capita, GDP per capita, Executive Constraints and Polity are all lagged one period. The Participation variable is measured at the beginning of each period and is made up of the ten participation concepts identified in the POLITY data (Marshall and Jaggers, 2006). One represents the least participatory regime and ten the most.

Table 13 presents the first set of exogenous variables, which appear in the instrumental variables regressions without fixed effects because they are all time-invariant.[32] The first is a measure of the percentage of the population in Koeppen-Geiger temperature zones from the Center for International Development (CID) at Harvard University.[33] Next are indicators for the three main colonial powers and

[32] Other exogenous variables were initially included as well but were dropped due to insignificance.
[33] See www.cid.harvard.edu

indicators for exports of oil or diamonds. The final two variables measure the polity score and cereal yield at independence.

Table 13: Exogenous Variables in IV Regressions

Variable	Source	Mean	Std Dev	Min	Max
Population in K-G temp zone (%)	CID	10.62	25.43	0.00	98.00
British Colony	World Factbook	0.34	0.48	0.00	1.00
French Colony	World Factbook	0.49	0.50	0.00	1.00
Portugese Colony	World Factbook	0.04	0.20	0.00	1.00
Oil	World Factbook	0.27	0.45	0.00	1.00
Diamonds	World Factbook	0.11	0.32	0.00	1.00
Initial Polity Score	POLITY IV	-3.18	5.11	-9.00	8.00
Initial Cereal Yield	WDI	858.16	549.34	264.62	3316.77

Table 14 presents exogenous variables that vary across countries and over time. The first two measure the change in temperature and precipitation as a percentage of the country average, which come from the Tyndall Centre for Climate Change Research.[34] Population density measures people per square kilometer and Years independent is measured at the beginning of each period. The spatial lags of other endogenous variables were created in the same way as the Polity spatial lag described earlier.

Table 14: Exogenous Variables that Vary Across Countries and Over Time

Variable	Source	Mean	Std Dev	Min	Max
Change in Temperature (% of avg.)	Tyndall	0.19	1.20	-3.45	3.34
Change in Precipitation (% of avg.)	Tyndall	-0.60	13.40	-47.85	40.36
Population Density (lagged one per.)	WDI	36.88	45.66	0.89	286.43
Years Independent		23.26	11.07	2.00	54.00
Civil Violence Spatial Lag	MEPV	0.03	0.02	0.00	0.12
Polity Spatial Lag (lagged one per.)	POLITY IV	-0.07	0.04	-0.17	0.03
GDP pc Spatial Lag (lagged one per.)	WDI	10.39	3.36	2.49	27.15
Savings Spatial Lag	WDI	0.19	0.05	0.06	0.40
Trade Spatial Lag (lagged one per.)	WDI	1.00	0.31	0.29	1.92
Death Rate Spatial Lag	WDI	0.26	0.07	0.12	0.48

[34] The data is available from http://www.cru.uea.ac.uk/~timm/data/index-table.html

Equation 1 is the basic specification for Growth:

$$y_{it} = \beta x_{it} + \gamma z_{it} + \mu_{it} \quad (1)$$

where x_{it} is a vector of endogenous covariates and z_{it} a vector of exogenous covariates. In a panel data model, $\mu_{it} = c_i + t_t + \varepsilon_{it}$, where c_i and t_t are the country and time fixed effects, respectively, and ε_{it} is the true stochastic error term. Estimating (1) without time and country fixed effects assumes that c_i and t_t are orthogonal to x_{it} and z_{it}. If this assumption is false, the estimates of β and γ are biased; if it is true then the estimates of β and γ will not change when fixed effects are included.

Determining which variables belong in z_{it} can be settled empirically since the variables are exogenous. The strategy I use is simply to start with a broad set of variables and drop those that are clearly insignificant, so long as their omission does not have any clear effects on other estimated coefficients. Determining which variables belong in x_{it} requires much greater consideration since instruments are not available for most endogenous covariates.

I consider five endogenous covariates in equation (1). The first is Executive Constraints, which is the variable of interest due to the expected positive relationship between Executive Constraints and Growth identified in the empirical literature on institutions (Acemoglu and Johnson, 2005). The second is Savings, which is a traditional component of capital-accumulation growth models and therefore can be expected to increase growth. The third is Trade, which has been a key driver of growth in many countries and is therefore also expected to have a positive effect on growth. The fourth is GDP per capita, which is included because the traditional growth model suggests countries starting at lower levels of GDP per capita should grow faster initially, the catch-up effect. I thus expect a negative estimated coefficient

on GDP per capita. The last endogenous variable in (1) is Aid per capita. While the effectiveness of international aid in Africa is hotly contested, the intentions of aid include political and economic stabilization, compensation from adverse shocks and laying foundations for development, all of which should have a positive impact on growth.

The estimations for Death Rate and Civil Violence take the same form as equation (1), although with slightly different endogenous variables. The Death Rate estimation includes Polity rather than Executive Constraints since the hypothesis is that greater democracy leads to better public goods. The Death Rate estimation includes Aid per capita, which may be directly targeted at diseases such as malaria, and GDP per capita as an indicator of a country's ability to provide public goods. Savings and Trade are not included in the Death Rate Equation.

The Civil Violence estimation includes Participation as the institutional variable of interest and Growth, GDP per capita and Aid per capita are the other endogenous variables present. Growth is present because negative growth shocks have been shown to increase the likelihood of civil conflict (Miguel et al, 2004). GDP per capita is included because it is thought that persistent poverty may lead to conflict and that countries with higher per capita GDP will thus be less prone to civil war. Finally, Aid per capita is included again because Aid is often targeted at crisis areas with the intention of obviating or alleviating pressures that might lead to violence.

I instrument for Executive Constraints, Polity and Participation with the spatial lag of Polity, as discussed above, as well as other relevant exogenous variables such as indicators for colonial masters and the Cold War and the initial Polity score for each country. Table 15 presents the first stage estimates on each of the three institutional variables.

Table 15: First Stage Estimates

	Executive Const.	Polity	Participation
Polity Spatial Lag	**9.05*** (3.30)**	**29.98** (12.91)**	**21.12*** (7.56)**
British Colony	0.51 (0.34)	1.11 (1.20)	0.13 (0.56)
French Colony	**0.73*** (0.22)**	1.37 (0.94)	0.76 (0.49)
Portuguese Colony	**1.45*** (0.27)**	0.69 (1.62)	-0.55 (1.08)
Coldwar	-0.30 (0.18)	-1.00* (0.55)	**-0.96*** (0.33)**
Initial Polity	**0.14*** (0.03)**	**0.43*** (0.13)**	**0.17** (0.07)**
Percent in K-G Temperate Zone		**-3.47** (1.36)**	
Initial Cereal Yield		**-0.001* (0.0007)**	
Trade spatial Lag		1.93 (1.19)	**2.31*** (0.80)**
Oil			-0.55 (0.38)
Diamond			1.15 (1.09)
R-squared	0.32	0.35	0.35

The coefficient on the Polity Spatial Lag is positive and significant in each estimation. Other exogenous variables were dropped when insignificant and when excluding them had no strong effect on any other coefficient. Overall, the first stage estimates are consistent with a valid instrument.

Table 16 presents four IV estimates on Growth. The first two specifications exclude other endogenous variables; the first includes fixed effects and the second does not. The third and fourth include other endogenous variables. The most striking result in Table 16 is that the estimated coefficient on Executive Constraints is significant only when fixed effects are not included. It is marginally insignificant (p-value of .13) without fixed effects when endogenous covariates are included, but it is clearly insignificant when fixed effects are present. The estimated coefficients on Savings and GDP per capita are significant and have the expected signs with and without fixed effects. The estimated coefficient on Aid per capita is significant only without fixed effects and the coefficient on Trade is never significant.

The estimated coefficient on change in precipitation is positive and significant in three of the specifications. The estimated coefficient on change in temperature is never close to being significant. The results on Growth indicate that in the presence of time and country fixed effects the estimated coefficient on Executive Constraints is

clearly insignificant. I next consider Death Rate to see if the effect is the same. Table 17 presents four regressions on Death Rate in the same form as Table 16.

Table 16: IV Regressions on Growth with and without Fixed Effects

Fixed Effects?	yes	no	yes	no
Endogenous Covariates?	no	no	yes	yes
Executive Constraints	0.072	**0.754**	-0.015	0.610
(std. err)	1.60	0.45	1.68	0.39
(p-value)	0.96	0.10	0.99	0.13
Change in Temperature	-0.044	-0.001	0.025	0.006
(std. err)	0.11	0.13	0.12	0.14
(p-value)	0.69	0.99	0.84	0.96
Change in Precipitation	**0.020**	**0.023**	0.016	**0.021**
(std. err)	0.01	0.01	0.01	0.01
(p-value)	0.09	0.02	0.18	0.04
Savings			**0.050**	**0.088**
(std. err)			0.03	0.03
(p-value)			0.07	0.00
Trade			0.003	-0.006
(std. err)			0.02	0.01
(p-value)			0.86	0.52
GDPpc			**-0.006**	**-0.001**
(std. err)			0.00	0.00
(p-value)			0.00	0.00
Aid per capita			0.006	**0.016**
(std. err)			0.01	0.01
(p-value)			0.57	0.08
R-Squared	0.35	0.19	0.51	0.23

The results in Table 17 indicate that the estimated coefficients on every variable change when fixed effects are included. The effects of Polity are insignificant in every specification, although the estimated coefficient is negative without fixed effects and positive with them. The estimated coefficients on Aid and GDP per capita are significant with the expected sign without fixed effects but are clearly insignificant with them. The coefficient on the Death Rate spatial lag is the only estimate that is close to significance when fixed effects are included.[35] The Death Rate IV regressions indicate again that fixed effects are necessary.

[35] Interestingly, the estimated coefficient on Polity is negative and significant without fixed effects when the Death Rate spatial lag is not included. This result indicates that

Table 17: IV Regressions on Death Rate with and without Fixed Effects

Fixed Effects?	*yes*	*no**	*yes*	*no**
Endogenous Covariates?	no	no	yes	yes
Polity	0.464	-0.112	0.451	-0.114
(std. err)	0.40	0.25	0.40	0.20
(p-value)	0.25	0.66	0.26	0.56
Death Rate Spatial Lag	12.088		12.290	
(std. err)	7.99		7.98	
(p-value)	0.14		0.13	
Aid per capita			0.008	**-0.028**
(std. err)			0.01	0.02
(p-value)			0.51	0.10
GDP per capita			0.000	**-0.001**
(std. err)			0.00	0.00
(p-value)			0.90	0.10
R-squared	0.88	0.63	0.88	0.66

Estimates without fixed effects included a number of time-invariant and country-invariant controls

I turn now to estimates on Civil Violence in Table 18. The Civil Violence estimates also indicate that fixed effects are necessary. The estimated coefficient on Participation is negative and significant without fixed effects but is insignificant when fixed effects are present. The estimated coefficients on population density and the civil violence spatial lag are consistent across specifications, while the coefficients on Growth and Aid change when fixed effects are present. The coefficient on change in precipitation has the expected sign but is only significant in one specification while the coefficient on GDP has the expected sign and is significant with and without fixed effects. The presence of fixed effects in the Civil Violence regressions does not change every coefficient, but it does render the estimated coefficient on Participation insignificant.

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the regression without the spatial lag is underspecified and thus subject to omitted variable bias due to the omission of the spatial lag.

Table 18: IV Regressions on Civil Violence with and without fixed effects

Fixed Effects?	yes	no	yes	no
Endogenous Covariates?	no	no	no	no
Participation	-0.414	**-0.939**	-0.300	**-0.699**
(std. err)	0.49	0.53	0.41	0.30
(p-value)	0.40	0.08	0.47	0.03
Change in Precipitation	-0.007	**-0.002**	-0.005	-0.001
(std. err)	0.01	0.00	0.01	0.00
(p-value)	0.35	0.04	0.54	0.20
Population Density	**0.046**	**0.022**	**0.044**	**0.011**
(std. err)	0.03	0.00	0.02	0.01
(p-value)	0.07	0.00	0.08	0.06
Civil Violence Spatial Lag	**-66.96**	**-68.675**	**-70.35**	**-68.304**
(std. err)	20.82	20.18	20.32	17.73
(p-value)	0.00	0.00	0.00	0.00
Growth			**-0.104**	-0.031
(std. err)			0.05	0.05
(p-value)			0.06	0.50
Aid per capita			-0.020	**-0.042**
(std. err)			0.02	0.01
(p-value)			0.26	0.01
GDP per capita			**-0.001**	**-0.001**
(std. err)			0.00	0.00
(p-value)			0.10	0.04
R-squared	0.63	0.30	0.64	0.31

The estimations presented above indicate that country and time fixed effects capture important variation that renders the effects of institutional variables insignificant. Fixed effects capture unobserved characteristics of the units of analysis; their importance here suggests that the equations without fixed effects are omitting significant factors that impact both institutions and the dependent variables.

The data are a three year panel while many of the major papers showing significant effects of institutions use a cross section of countries to analyze the long run determinants of GDP per capita (Acemoglu, Johnson and Robison 2001, Acemoglu and Johnson 2005, Rodrik et al. 2004). One might be tempted to argue then that those estimations are substantially different and fixed effects are not needed in them. Such an argument is incorrect. A panel is simply cross sections stacked over

time. Without fixed effects, a panel estimation is equivalent to the cross section data used in those papers, only with a larger sample. It is evident in Tables 16 and 18 that the institutional variables are consistently significant without fixed effects, much as they have been found to be in the IV estimates in the literature. Given the vast changes in the estimated coefficients on institutions when fixed effects are introduced, one must presume that the earlier estimates in the literature would change in a similar fashion.

It is for this reason that I claim the literature is now moving into a third phase where fixed effects must be accounted for. The most straightforward way to estimate the effects of institutions while including fixed effects is to use an instrument that varies across countries and over time, as in the above estimates. However, there are still many limitations to the estimates presented above. The biggest is other endogenous covariates. The IV literature tends to exclude other endogenous variables, but the fixed effects results suggest that such omission may be a source of bias. Of course, including the endogenous variables without instruments also may introduce bias. The only other alternative is to account for all of the endogenous variables in a simultaneous equations model.

V. Simultaneous Equations Estimation

The technique used here is best illustrated through a simple example. Consider a two equation model with two endogenous variables, x_1 and x_2, and two exogenous variables, z_1 and z_2. Suppose the model is as follows:

$$x_1 = \alpha_{11}x_2 + \alpha_{12}z_1 + \mu_1 \quad (2)$$

$$x_2 = \alpha_{21}x_1 + \alpha_{22}z_2 + \mu_2 \quad (3)$$

The basic idea is to use substitution to solve (2) and (3) so that each is written only as a function of the exogenous variables. Such substitution yields the following coefficients on the exogenous variables:

	z_1	z_2
x_1	$\alpha_{12}/(1-\alpha_{11}\alpha_{21})$	$\alpha_{11}\alpha_{22}/(_1-\alpha_{11}\alpha_{21})$
x_2	$\alpha_{21}\alpha_{12}/(1-\alpha_{11}\alpha_{21})$	$\alpha_{22}/(1-\alpha_{11}\alpha_{21})$

Suppose now that we regress both x_1 and x_2 each on z_1 and z_2. We have avoided endogeneity in the regression and we can now solve for the coefficients in equations (2) and (3). Denote the regression coefficients by π_{ij}. Algebraic substitution gives that $\pi_{21}/\pi_{11} = \alpha_{21}$ and $\pi_{12}/\pi_{22} = \alpha_{11}$. Substituting those values into π_{11} and π_{22} gives α_{12} and α_{22} and the system is solved.

In the simple example above the system is exactly identified: there are four structural parameters from (2) and (3) and four reduced form estimated coefficients, which yields a unique solution. In the estimates I present below the system is overidentified so there are multiple possible solutions. However, the basic strategy is the same. I start by regressing each of the ten endogenous variables in Table 3 on the ten exogenous variables in Table 5 and country and time fixed effects. A simultaneous equations estimator such as GLS is appropriate; however, since the right hand side of each equation is the same GLS is equivalent to equation by equation OLS, which I estimate (Greene 2003). The results are 100 reduced form estimated coefficients, which I stack into a vector denoted Π. The country and time fixed effects are included in the first stage estimates but not in the vector Π.[36]

The structural parameters, denoted α_{ij}, are determined by which endogenous variables are included and which exogenous variables are excluded from each equation in the model. For Growth, Death Rate and Civil Violence the model includes the endogenous variables presented in the IV estimates. These endogenous variables are the other seven endogenous variables in the model, and each has its own structural

[36] The fixed effects coefficients could be added to Π, but would simply complicate solving the model without affecting the variables of interest, which are identified from the exogenous variables in the model.

equation, which are presented in Appendix 9. The structural model is solved through algebraic substitution as in the example above. The result is a 10 x 10 matrix that assigns the structural parameters for each reduced form coefficient, again as in the example above. I stack this matrix into a vector, which is denoted A. The system can now be written as a minimum distance problem:

$$\min_{\alpha} (\Pi - A)' \Omega^{-1} (\Pi - A) \quad (4)$$

where Ω is the variance of Π. Simultaneous equations estimation has rarely been used in the literature, but it has several advantages.[37] The parameters of interest on the endogenous covariates are identified from a set of exclusion restrictions in the structural model. In the example above, z_2 is excluded from (2) and z_1 is excluded from (3). Without these restrictions the model could not be solved; they are similar to the exclusion restrictions required in IV estimation. However, the minimum distance framework in (4) gives a test of the overidentifying restrictions of the model (Jakubson 2008). When the minimum distance problem is weighted by the inverse variance of the unrestricted vector, as in (4), the minimum distance estimator is efficient and the objective function, denoted $d(\alpha)$, has the following distribution:

$$n * d(\alpha) \sim chi^2 (q-p) \quad (5)$$

where n is the number of observations, q is the number of reduced-form (unrestricted) coefficients and p is the number of parameters (restrictions) in A (Jakubson 2008). This simultaneous equations approach is thus similar to the IV approach in that exclusion restrictions are necessary for identification, but it has the advantage of providing a test of the restrictions, which is not possible in IV estimation. The simultaneous equations model also allows for exploration of other pathways through which institutions might affect Growth, Death Rate and Civil Violence. For

[37] With the notable exception of Ngelaza et al. (2006), whose excellent paper uses simultaneous equations and also carefully constructs spatial variables.

example, the structural equation for Savings includes Death Rate, Executive Constraints and Civil Violence, so it allows us to consider if those endogenous variables affect Growth indirectly through savings.

The model I estimate has 100 unrestricted reduced form coefficients in Π and 90 structural parameters in A. The weight matrix Ω is the variance matrix from the reduced form simultaneous equations regressions. Since Ω is calculated from the reduced form residuals, serial or spatial autocorrelation in the first stage affects Ω, which can then bias the structural estimates. I therefore carefully examine each first stage residual for serial and spatial autocorrelation and correct nine residuals for serial autocorrelation and five for spatial autocorrelation.[38] I estimate the structural model by solving (4); the resulting test statistic $n*d(\alpha) \sim chi^2(10)$ equals 9.86, indicating no evidence against the overidentifying restrictions in the model. Table 19 presents the simultaneous equations estimates on Growth.

Table 19: Simultaneous Equations Estimates on Growth

	Coefficient	Std. Error	p-value
Executive Constraints	**1.3838**	0.204	0.000
Savings	0.0161	0.014	0.257
Trade	0.0080	0.032	0.803
GDP per capita	-0.0004	0.003	0.877
Aid per capita	**0.1098**	0.060	0.069
Change in Precipitation	0.6326	3.998	0.874
Change in Temperature	-0.1289	6.758	0.985

The results indicate that Executive Constraints do have a significant and large positive effect on Growth. A one point increase in the Executive Constraints score (which ranges from one to seven) is estimated to increase Growth on average by 1.38 percent. Savings has a positive estimated coefficient but is insignificant and the estimated coefficients on Trade, GDP per capita, Precipitation and Temperature are

[38] The details can be found in Appendix 10.

clearly insignificant. The estimated coefficient on Aid per capita is positive and significant. It indicates that an increase in Aid per capita of about $9.00 would on average increase Growth by one percent. This is a large estimated effect of Aid on Growth. Rajan and Subramanian (2007) for example, survey the literature on Aid and Growth and argue that a realistic empirical effect of Aid/GDP on Growth should range from 0.03 to 0.16. In those terms, the estimated result here is equivalent to about 0.77. These results differ markedly from the IV estimates with fixed effects, especially on the Executive Constraints variable.

Table 20: Simultaneous Equations Estimates on Death Rate

	Coefficient	Std. Error	p-value
Polity	**-0.2179**	0.038	0.000
Aid per capita	**0.1363**	0.057	0.016
GDP per capita	0.0001	0.006	0.982
Death Rate Spatial Lag	**20.3974**	5.261	0.000
Trade Spatial Lag	**-8.1092**	0.992	0.000
Savings Spatial Lag	**-10.8467**	1.550	0.000
Change in Precipitation	-3.8364	7.196	0.594
Change in Temperature	0.2901	4.120	0.944

Table 20 presents the simultaneous equations results on Death Rate. The estimated coefficient on Polity is negative and significant, indicating that a one point increase in the polity score would on average lower the Death Rate by 0.22 people per thousand. This estimate differs greatly from the IV estimates, where the coefficient on Polity was never significant. The estimated coefficient on Aid per capita is surprisingly positive and significant. The coefficient on the Death Rate Spatial lag is positive and significant, which is consistent with the fixed effects IV results. The Death Rate results do indicate a positive effect of democracy and differ significantly from the IV results.

Table 21: Simultaneous Equations Estimates on Civil Violence

	Coefficient	Std. Error	p-value
Participation	-0.7935	0.902	0.379
Growth	0.4003	0.383	0.296
GDP per capita	0.0051	0.009	0.563
Aid per capita	-0.3675	0.325	0.258
Civil Violence Spatial Lag	**-41.9491**	5.592	0.000

Table 21 presents the simultaneous equations estimates on Civil Violence. The estimated coefficients on Participation, Growth and GDP per capita are all insignificant. The insignificance of the Growth and GDP per capita coefficients differs from the IV results where both coefficients were negative and significant. The only significant coefficient is the estimate on the Civil Violence spatial lag, which is surprisingly negative. This coefficient is also significant and negative in the IV estimation, indicating a consistent result that civil violence in neighboring countries lowers contemporaneous civil violence in a given country. This result contradicts the common notion that civil violence is a chaotic force likely to spread across countries; however, the positive result may simply be due to the contemporaneous variables. Further regressions (not reported) that add in a temporal lag of the Civil Violence spatial lag find that it has a positive estimated coefficient, indicating that there may be a chaotic spread of civil violence but it takes a few years to make a significant impact. It is possible that the negative estimated coefficient on the contemporaneous variable is indicative of government action to clamp down quickly on civil violence when conflict breaks out in neighboring countries.

The simultaneous equations results for the other endogenous variables are in Appendix 9. The most interesting result from the other equations is that Executive Constraints is found to have a large significant positive effect on GDP per capita; an increase of one in the Executive Constraints score is estimated to increase GDP per capita by $104, significant at the one percent level. GDP per capita, however, is not estimated to have any significant effect on the Executive Constraints score. These

results are consistent with institutions causing long-term growth and inconsistent with institutions evolving naturally along with GDP (Paldam and Gundlach, 2008)

VI. Discussion and Conclusion.

Given the ongoing wave of democratization in Africa, this paper asked what the effects of democracy might be. Specifically, I considered if democracy would lead to economic growth through better institutions, if democracy would lead to a lower death rate and if democracy would reduce civil conflict through increased participation.

I estimate the effect of institutions on three variables of interest with both instrumental variables and simultaneous equations regressions. It is clear from the IV estimates that fixed effects are necessary. The estimated coefficients on most variables change drastically when fixed effects are introduced, indicating that estimates without fixed effects are likely biased. I find no significant effects of the institutional variables in the IV estimates. These results call into question much of the earlier literature on institutions and growth, which uses cross-sectional data and thus cannot include fixed effects.

The simultaneous equations estimation, which also includes country and time fixed effects, finds significant effects of institutions on Growth and Death Rate. The estimated effect of democracy on Death Rate is not large, but the estimated effect of Executive Constraints on Growth is very large. The simultaneous equations estimates also indicate that Executive Constraints has a large positive effect on GDP per capita. These results indicate that the strongest effect of the wave of democratization in Africa may be increased economic growth through improved institutions.

The simultaneous equations estimates differ from the IV estimates. I find the simultaneous equations model to be more believable because it accounts for the endogeneity of all ten endogenous variables and the test of the overidentifying

restrictions in the model indicates no evidence against those restrictions. These results thus indicate that simultaneous equations estimation may be a more productive method for future research.

The blossoming of democracy in Africa since the end of the Cold War is intrinsically good. I find that it also is instrumentally good. Improvements in democracy are estimated to lower the Death Rate and also lead to much stronger economic growth through better institutions.

APPENDIX 1: CONSTRUCTION OF INSTITUTIONAL VARIABLES AND COMPARISON WITH OTHER MEASURES

To create the regional institutional variables, I first identified variables of interest from the Afrobarometer survey. These variables are all measured on an ordinal scale. For example, the question may be "do you trust your local representative?" and the answers are "always," "most of the time," "sometimes," "rarely," or "never." I took the variables and calculated the percentages of each response by region. I also grouped the responses in different combinations. For example, I calculated the percentage of people who "always" or "most of the time" trust their local representative, and the percentage of people who "always," "most of the time" and "sometimes" trust their representatives. Thus each ordinal question from the survey yields five to seven potential variables. I selected the potential variable with the highest variance across all regions as the regional measure. This method effectively captures the variation in the data without arbitrarily indexing the responses.

Since I am using subjective variables as regional indicators of corruption, contract enforcement, etc., some comparison of the variables used here with other common measures of governance is needed. Unfortunately, regional data on governance in Africa is not available. Thus I computed the country-level variables for each of the Afrobarometer variables for all thirteen countries (with the exception of Law, which is replaced here by Law-tax) and compared them with Freedom House scores and selected indices from the Mo Ibrahim index of governance.[39] Tables A1

[39] See www.freedomhouse.org and www.moibrahimfoundation.org respectively for more information on these measures. Law-tax replaces Law here because factor scores created from a sample of 13 would be substantively different from those created from a sample of 151, whereas Law-tax is a percentage measure and is comparable across different levels of aggregation.

and A2 present the correlations of the Afrobarometer indicators with the Mo Ibrahim measures and Freedom House Scores, respectively.

Table A1: Comparison of Afrobarometer and Mo Ibrahim Measures

		Crime	Law-tax	Trust Army	Trust Local Rep	Reps Corrupt	Police Corrupt
Mo Ibrahim	Contracts	0.53	-0.14	-0.44	-0.38	-0.27	-0.22
Mo Ibrahim	Safety	-0.57	0.25	0.40	0.48	-0.44	-0.62
Mo Ibrahim	Corruption	-0.09	-0.28	-0.17	-0.15	-0.26	-0.45
Mo Ibrahim	Participation	-0.17	0.06	-0.03	-0.04	-0.45	-0.46
Mo Ibrahim	Rule of Law etc.	-0.10	-0.20	0.00	-0.05	-0.42	-0.42

The variables from the Mo Ibrahim (hereafter MI) foundation are all indices that range from zero to 100, and all are scaled so that higher scores are better. Thus the expectation is that Crime, Reps Corrupt and Police Corrupt will all be negatively correlated with the MI measures, and strongly so with those that are closest to what the Afrobarometer variables measure. Law-tax and the trust variables should generally be positively correlated with the MI variables.

Crime is negatively correlated with MI safety, as expected, but is surprisingly positively correlated with MI contracts. Law-tax has no large correlations and is surprisingly negatively correlated with MI contracts and MI rule of law etc. The trust variables are moderately correlated with MI safety, but negatively correlated with MI contracts. It seems that MI contracts is not a clear variable. Reps Corrupt and Police Corrupt are negatively correlated with all the MI variables as expected.

Table A2: Comparison of Afrobarometer and Freedom House measures

	Crime	Law-tax	Trust Army	Trust Loc. Rep	Reps. Corrupt	Police Corrupt
FH Political Rights	0.00	0.56	0.21	0.26	0.13	0.17
FH Civil Liberties	-0.06	0.51	-0.07	-0.01	0.11	0.02

Freedom House scores range from one to seven, with one being the best score. Thus I expect positive correlations with Crime and the corruption variables and negative correlations with Law-tax and the trust variables. However, the correlations in general are low. The one surprising result is that Law-tax is moderately positively correlated with both Freedom House indices, which implies that higher expectations of taxes being collected are associated with fewer political rights and civil liberties.

The comparison with other measures of governance, while limited to a thirteen country sample, does offer some insight. The corruption variables are the only Afrobarometer measures that consistently correlate negatively with the MI indices, which suggests that they are perhaps better measures of governance than the trust variables. The correlations with the Crime and Law-tax variables are not as strong as expected, and the positive correlation of Law-tax with the Freedom House scores indicates that this variable may capture government heavy-handedness.

The comparison also points out the strengths of the Afrobarometer variables and the methods used to create them. For example, it seems that the MI contracts index is problematic – its correlations go the wrong way with most of the Afrobarometer variables. Since it is an index, it is not clear if the problem is with the variable itself or is a result of the arbitrary weights used to scale it. The positive correlations of Law-tax with the Freedom House scores are surprising as well, but since it is clear what Law-tax measures – the percentage of people who expect the law to be enforced if they do not pay taxes – it is possible to explain the surprising result.

APPENDIX 2: MINIMUM DISTANCE FACTOR ANALYSIS

The basic idea of factor analysis is that if one has many variables that are indicators of a single underlying unobserved variable, a structure is implied on the variance covariance matrix of the indicators. Suppose that the model is

$$y_i = \beta_i u + \varepsilon_i \qquad \text{(A1)}$$

where y is the indicator and u is the unobserved variable. This model implies a certain structure. For example, if i = 4 and we assume that $\sigma_{\varepsilon i \varepsilon j} = 0$ for all i,j, then the variance-covariance matrix V has the following structure:

$\beta_1^2\sigma^2_u+\sigma^2_{\varepsilon 1}$	$\beta_1\beta_2\sigma^2_u$	$\beta_1\beta_3\sigma^2_u$	$\beta_1\beta_4\sigma^2_u$
$\beta_2\beta_1\sigma^2_u$	$\beta_2^2\sigma^2_u+\sigma^2_{\varepsilon 2}$	$\beta_2\beta_3\sigma^2_u$	$\beta_2\beta_4\sigma^2_u$
$\beta_3\beta_1\sigma^2_u$	$\beta_3\beta_2\sigma^2_u$	$\beta_3^2\sigma^2_u+\sigma^2_{\varepsilon 3}$	$\beta_3\beta_4\sigma^2_u$
$\beta_4\beta_1\sigma^2_u$	$\beta_4\beta_2\sigma^2_u$	$\beta_4\beta_3\sigma^2_u$	$\beta_4^2\sigma^2_u+\sigma^2_{\varepsilon 4}$

Under the critical assumption that $\sigma_{\varepsilon i \varepsilon j} = 0$ for all i,j, there are in this case nine structural variables and ten unique entries in V, so the system appears to be identified. However, σ^2_u is never observed separately from one of the βs, so we cannot yet identify all the parameters. It is necessary to make some normalization, so we normalize $\beta_1=1$, and then we can identify all of the other parameters in the system.

The parameters are then estimated by minimum distance. We take the computed variance covariance matrix and stack the upper right triangle into the vector Π. We then stack the upper right triangle of V as well into the vector v and choose the parameter set Θ to minimize the distance between Π and v. The problem is:

$$\min_{\Theta} (\Pi - v)' \Omega (\Pi - v)$$

For efficient minimum distance, the matrix Ω should be the inverse variance of Π, that is the inverse variance of the sample variance. Solving this problem gives parameter estimates as well as a test of fit of the model. The parameter estimates are then used to create the factor scores. If we let G be the estimated matrix from the

parameters, b be the vector of factor loadings for each indicator and X be the matrix of data, the factors scores are then given by:

$$\text{Scores} = X*G^{-1}*b*\sigma^2_u \qquad \text{(A2)}$$

In the analysis of this paper, I used the following indicators of wealth to create the wealth factor scores: radio, television, refrigerator, car/truck, and dirt floor. The coefficients for each of these indicators were estimated by a model exactly as presented above. The coefficient for the radio indicator was normalized to one and the factor scores were then computed from the estimated parameters. This same technique was used to create the public goods factor score and the Law variable. Table A3 presents the factor loadings for each of the three factor scores used in the analysis.

Table A3: Factor Analysis Results

Public Goods	Loading	Law	Loading	Wealth Factor	Loading
Flush Toilet	1.000	Law-Tax	1.000	Radio	1.000
Electricity	1.400	Law-Crime	0.122	Television	1.744
Telephone	0.637	Courts Bind*	0.327	Refrigerator	1.201
Piped Water	1.338	Trust Courts	0.236	Car/Truck	0.575
Dirt Floor	-0.546	Judges Corrupt	-0.248	Dirt Floor	-1.068
$\sigma^2_{\varepsilon 1}$	0.010	$\sigma^2_{\varepsilon 1}$	0.000	$\sigma^2_{\varepsilon 1}$	0.224
$\sigma^2_{\varepsilon 2}$	0.029	$\sigma^2_{\varepsilon 2}$	0.000	$\sigma^2_{\varepsilon 2}$	0.041
$\sigma^2_{\varepsilon 3}$	0.004	$\sigma^2_{\varepsilon 3}$	0.006	$\sigma^2_{\varepsilon 3}$	0.054
$\sigma^2_{\varepsilon 4}$	0.008	$\sigma^2_{\varepsilon 4}$	0.010	$\sigma^2_{\varepsilon 4}$	0.049
$\sigma^2_{\varepsilon 5}$	0.221	$\sigma^2_{\varepsilon 5}$	0.056	$\sigma^2_{\varepsilon 5}$	0.213
σ^2_u	0.052	σ^2_u	0.068	σ^2_u	0.043
Chi2(5)	1979.075	Chi2(5)	16735.901	Chi2(5)	2919.562

*Courts Bind measures the percentage of people who agree or strongly agree that court rulings should bind

Note that the loadings in table A1 are not the same as the actual weights used to create the factor scores. The loadings represent the β's from equation A1, that is they represent the effect of wealth (or public goods, or court systems) on the indicator. The actual index weights used to create the variables used in the paper are given by:

$$\text{Weights} = G^{-1}*b*\sigma^2_u \qquad \text{(A3)}$$

Table A4 presents the index weights applied to each of the three variables created through this method in the paper. The interesting finding here is that for the

Law variable, the weights on the last three variables that go into it are zero. Thus this variable is actually a weighted average of only two factors, Law-tax and Law-crime, because the results of the factor model are that the other three indicators of court functioning have a weight of zero.

Table A4: Index Weights for Factor Analysis Variables

Public Goods Variables	Index Weights	Law Variables	Index Weights	Wealth Variables	Index Weights
Flush Toilet	0.19	Law-Tax	0.50	Radio	0.03
Electricity	0.09	Law-Crime	4.38	Television	0.27
Telephone	0.31	Courts Bind	0.00	Refrigerator	0.16
Piped Water	0.33	Trust Courts	0.00	Car/Truck	0.08
Dirt Floor	0.00	Judges Corrupt	0.00	Dirt Floor	-0.05

APPENDIX 3: CONCERNING THE ENDOGENEITY OF REGIONAL LEVEL VARIABLES ON HOUSEHOLD WEALTH

The concern is often raised that the technique used in this paper of regressing household (lower level) wealth on regional (upper level) institutional variables does not avoid the problem of endogeneity because the institutional variables are identified off of the regional means of the household variables. If this concern is valid, the estimations here are equivalent to regressing regional means of household variables on regional institutional variables. I show that when household controls are included regressing household variables on regional institutional variables is different than regressing regional means on the regional institutional variables, and further that the coefficients on the regional institutional variables are identified off of both household and regional level data, not just off of the regional means of the household data.

Proof: Let y be the 1 x n dependent variable at the household (lower) level and let X be a 2 x n matrix of independent variables where X_1 is a household level variable and X_2 is a regional (upper) level variable.

We begin with the least squares estimator

$$\beta = (X'X)^{-1}(X'y) \qquad (1)$$

Expansion of (1) gives

$$\beta_2 = [X_1'X_2X_2'y - X_2'X_1X_1'y]/\det(X'X) \qquad (2)$$

β_2 is the coefficient on the regional variable. Now consider two cases. In the first case, let y and X_1 be the regional means of the household variables. In the second case, let y and X_1 remain household variables. In case one, we have

$$X_1'X_2X_2'y = [X_{2r1}\Sigma_{r1}X_1/j+\ldots+X_{2rm}\Sigma_{rm}X_1/j]*[X_{2r1}\Sigma_{r1}y/j+\ldots+X_{2rm}\Sigma_{rm}y/j] \qquad (3)$$

$$X_2'X_1X_1'y = [X_{2r1}\Sigma_{r1}X_1/j+\ldots+X_{2rm}\Sigma_{rm}X_1/j]*[\Sigma_{r1}X_1\Sigma_{r1}y/j^2+\ldots+\Sigma_{rm}X_1\Sigma_{rm}y/j^2] \qquad (4)$$

Where r stands for region and there m regions, each of which has j households. In case two, we have

$$X_1'X_2X_2'y = [X_{2r1}\Sigma_{r1}X_1+\ldots+X_{2rm}\Sigma_{rm}X_1]*[X_{2r1}\Sigma_{r1}y+\ldots+X_{2rm}\Sigma_{rm}y] \quad (5)$$

$$X_2'X_1X_1'y = [X_{2r1}\Sigma_{r1}X_1+\ldots+X_{2rm}\Sigma_{rm}X_1]*[X_{11}y_1+X_{12}y_2+\ldots+X_{1n}y_n] \quad (6)$$

It is clear that the first term in β_2, $X_1'X_2X_2'y$, differs across the two cases only by a factor of j^2. That is (3) is equivalent to (5)/j^2. However, the second term in β_2, $X_2'X_1X_1'y$, is substantively different across the two cases. The first part, $X_2'X_1$, differs only by a factor of j, but the second part, $X_1'y$, is truly different. In case one $X_1'y$ consists of the regional means of X_1 times the regional means of y. Expanding these terms gives an expression that includes each x_{1i} multiplied by y_i and by y_j for all other j in the same region. In case two, the $X_1'y$ term consists of each x_{1i} multiplied only by y_i. Thus this last term differs substantively across the two cases. It follows that in general β_2 is different in the two cases and thus regressing regional variables on household data is not equivalent to regressing regional variables on regional means so long as household controls are included. The coefficient on the regional variable is identified from both regional and household variation, not just from regional variation and is therefore not endogenous (so long as the assumption that households treat institutions as exogenous holds).

APPENDIX 4: TABLE A5: FULL REGRESSION RESULTS

Estimation by weighted least squares, dependent variable is wealth factor score

Observations: 118,262 R-squared: 0.5259

Variable	Coefficient	Std. Err.	t-stat	p-value
Complete Primary Education	0.17	0.02	8.78	0.00
Incomplete Secondary Education	0.43	0.03	13.80	0.00
Complete Secondary Education	0.81	0.06	13.97	0.00
Higher Education	1.25	0.04	29.91	0.00
Age	0.02	0.00	8.63	0.00
Age Squared	0.00	0.00	-9.05	0.00
Female Household Head	-0.08	0.02	-4.90	0.00
Household in Urban Area	0.40	0.03	13.02	0.00
Law	-0.07	0.03	-2.04	0.04
Trust Army	0.96	0.26	3.73	0.00
Trust Local Rep	-0.90	0.28	-3.19	0.00
Contact Local Rep	1.76	0.66	2.67	0.01
Contact Local Rep Squared	-0.83	0.33	-2.52	0.01
Reps Corrupt	-0.96	0.47	-2.04	0.04
Police Corrupt	1.43	0.52	2.73	0.01
Police Corrupt Squared	-1.27	0.47	-2.74	0.01
Public Goods Factor Score	0.19	0.10	1.89	0.06
Crime*Law	0.14	0.08	1.64	0.10
Crime*Trust Army	-1.29	0.36	-3.58	0.00
Crime*Trust Local Rep	1.41	0.35	4.07	0.00
Crime*Ed Years	-0.08	0.02	-3.18	0.00
Crime*Urban	0.29	0.21	1.36	0.18
Law*Trust Army	0.11	0.04	2.70	0.01
Law*Public Goods Factor	0.05	0.02	2.53	0.01
Trust Army*Ed Years	-0.08	0.03	-2.48	0.01
Trust Local Rep*Police Corrupt	-0.95	0.42	-2.30	0.02
Trust Local Rep*Ed Years	0.12	0.02	4.94	0.00
Trust Local Rep*Urban	-1.44	0.33	-4.38	0.00
Contact Local Rep*Contact Nat Rep	-0.63	0.54	-1.17	0.24
Contact Local Rep*Urban	0.94	0.33	2.88	0.01
Contact Nat Rep*Reps Corrupt	1.20	0.62	1.95	0.05
Contact Nat Rep*Public Goods Factor	0.15	0.10	1.52	0.13
Reps Corrupt*Urban	-1.51	0.45	-3.34	0.00
Police Corrupt*Urban	1.24	0.36	3.48	0.00
Public Goods Factor*Urban	-0.09	0.03	-3.13	0.00
Constant	-1.58	0.21	-7.48	0.00

APPENDIX 5: OBJECTIVE CRIME MEASURES

There are two types of objective crime measures available in the survey: crime rates and indicators of the presence of security forces. These forces range from local police to the army, with several groups that fall in between. Please see Fafchamps and Moser (2003) for a more detailed discussion of crime in Madagascar.

We have crime rate data for the estimated numbers of murders per capita, break-ins per capita, and cattle thefts per capita. For security presence, we measure whether police, army, gendarmes, or quartiers mobile are present in a commune. Table A presents the results of three regressions using these variables. The first is a simple OLS regression on Employment with all of the infrastructure and institution variables that uses the crime rates rather than subjective security. The second is the same as the first but adds in all of the indicators of security presence. The only objective security variable that has a significant effect in either of these initial regressions is the Gendarmes indicator, so in the third regression we instrument for it and run a regression that is otherwise similar to the final specification in the paper. We find that Gendarmes has no significant effect there.

Table A6: Effects of Objective Crime Variables

Specification	OLS on Employment	OLS on Employment	IV Tobit on Employment
Variables			
Murders per capita	75.28 (1549.5)	-343.37 (1633.83)	
Break - ins per captia	92.64 (399.3)	91.40 (398.25)	
Cattle thefts per capita	-7.96 (12.6)	-12.90 (12.56)	
Gendarmes present		**2.37* (1.22)**	10.87 (13.28)
Quartiers Mobile present		-0.03 (1.90)	
Police present		1.83 (1.36)	
Military present		3.79 (2.53)	
Other variables	All other infrastructure and institution variables	All other infrastructure and institution variables	Institution and geography variables

APPENDIX 6: VARIANCE DECOMPOSITION

Consider a variable x with a variance V(x). The variable has 4 levels and we want to decompose the variance into the variance at each level. Denote by c the lowest level, d the next lowest, r the second highest and p the highest level. We need to define the variance at each level. The total variance is

$$V(x) = E(x_i - u)^2 \quad \text{(A1)}$$

where i is the ith observation and u is the mean. For the variance at the lowest level, c, we then define

$$V(c) = E(x_i - u_d)^2 \quad \text{(A2)}$$

where u_d is the mean of level d. Thus V(c) is the variance within the level m groupings and between the level s groupings. We similarly define the variance at levels d and r as

$$V(d) = E(u_d - u_r)^2 \quad \text{(A3)}$$

$$V(r) = E(u_r - u_p)^2 \quad \text{(A4)}$$

We are then left with the variance at the highest level, which by definition is

$$V(p) = V(x) - V(s) - V(m) - V(r) \quad \text{(A5)}$$

The variance decomposition presented in Table 2 was calculated from equations A2 through A5 where communes are the lowest level (c), districts are the second lowest (d), regions the second highest (r) and provinces the highest level (p).

APPENDIX 7: WITHIN AND BETWEEN ESTIMATIONS

Table A7: Within and Between Estimations

Dependent Variable: Employment	Between Estimation	Within Estimation	Between Estimation 2	Within Estimation 2
Institutional Variables				
Titled	**43.99** (19.32)**	**36.64* (19.94)**	**34.44*** (6.82)**	**39.36*** (6.80)**
Titled Squared	-15.14 (28.80)	4.18 (29.22)		
Security	-2.32 (1.52)	2.03 (1.61)		
Physical Infrastructure				
Paved Road	**8.07*** (1.83)**	**8.80*** (1.75)**	**8.07*** (1.80)**	**8.86*** (1.73)**
Unpaved Road	1.54 (1.50)	**-2.71* (1.47)**	1.59 (1.51)	-2.15 (1.47)
National Electricity	**15.67*** (3.51)**	**9.57** (3.79)**	**15.94*** (3.51)**	**9.84*** (3.77)**
Landline Phone Service	**12.04*** (2.94)**	**13.56*** (2.56)**	**11.73*** (2.92)**	**13.95*** (2.55)**
Cell Phone Service	3.04 (2.23)	3.10 (1.97)	2.48 (2.20)	2.88 (1.96)
Community Wells	**5.46*** (1.88)**	**3.63** (1.84)**	**5.16*** (1.87)**	**3.20* (1.85)**
Public Running Water	**13.41*** (3.32)**	**15.15*** (3.33)**	**13.64*** (3.32)**	**15.28*** (3.33)**
Number of Serv. Stations	**4.32** (1.80)**	**8.50*** (2.00)**	**5.59*** (1.97)**	**8.18*** (2.04)**
Number of Serv. Stations^2	**-0.18** (0.08)**	**-0.32*** (0.09)**	**-0.22*** (0.08)**	**-0.31*** (0.09)**
Market Infrastructure				
Number of Daily Markets	**4.56*** (1.01)**	1.34 (1.04)	**4.33*** (1.02)**	1.19 (1.13)
Number of Daily Markets^2	**-0.16*** (0.04)**	-0.03 (0.04)	**-0.15*** (0.04)**	-0.02 (0.05)
Number of Seasonal Mar.	-0.73 (0.47)	-0.42 (0.66)	-0.28 (0.18)	0.43 (0.28)
Number of Seasonal Mar.^{2}	0.02 (0.02)	0.04 (0.03)		
Formal Credit in Agriculture	-1.06 (1.33)	0.03 (4.93)		

APPENDIX 8: FIRST STAGE RESULTS

Table A8 below presents the first stage results for our main three endogenous variables: Titled, Security and Formal Credit in Agriculture. An OLS estimator was used on Titled, treating it as a continuous variable. An ordered probit would be more appropriate, but with only three instruments we could not identify each level of the ordered probit. Probits were used in the first stage estimates for Security and Formal Credit in Agriculture. The predicted value for each of these variables is then the probability that the variable equals one based on the probit coefficients.

Table A8: First Stage Results

First Stage Regressions	Titled	Security	Agricultural Credit
Estimator	OLS	Probit	Probit
Observations	2746	2714	2738
R^2 (OLS) / Pseudo R^2 (Probit)	0.107	0.073	0.073
French Presence	**1.14* (0.63)**	0.08 (0.07)	-0.09 (0.07)
French Administration	**1.37** (0.65)**	**-0.12* (0.64**)	-0.00 (0.07)
Foreigners Present	0.61 (0.54)	0.01 (0.06)	**0.14** (0.06)**
Ocean	0.58 (0.83)	**0.17** (0.09)**	0.06 (0.08)
River	-0.85 (0.50)	**0.09* (0.05)**	**-0.32*** (0.06)**
Forest	0.10 (0.72)	-0.02 (0.07)	**0.38*** (0.08)**
Distance to Urban Center	**-0.79*** (0.22)**	**-4.43* (2.66)**	**-21.22*** (1.90)**
Population	**1.15*** (0.31)**	2.86 (2.09)	2.70 (1.97)

APPENDIX 9: STRUCTURAL EQUATIONS AND OTHER SIMULTANEOUS EQUATIONS ESTIMATES

Each structural equation takes the following form:

$$y_{it} = \beta x_{it} + \gamma z_{it} + c_i + t_t + \varepsilon_{it} \quad \text{(A6)}$$

where x_{it} is a vector of endogenous variables z_{it} a vector of exogenous variables and c_i and t_t country and time fixed effects. The simultaneous equations model has ten endogenous and ten exogenous variables. Table A9 denotes which variables are in each remaining structural equation. The variables absent each structural equation form the exclusion restrictions that identify the model. Note that the purpose of this model is to fully specify the equations for Growth, Death Rate and Civil Violence, therefore the equations for the other endogenous variables have fewer endogenous variables and should not necessarily be considered fully specified.

Table A9: Structural Equations

	Savings	Trade	Aidpc	GDPpc	Ex.Const.	Polity	Part.
Death Rate	Yes	No	No	No	No	No	No
Civil Violence	Yes	No	No	No	No	No	No
Savings	-	No	No	No	No	No	No
Trade	No	-	No	Yes	No	No	No
Aid per capita	Yes	No	-	No	No	No	No
GDP per capita	Yes	No	No	-	Yes	No	No
Ex. Constraints	Yes	No	No	Yes	-	No	No
Polity	No	Yes	Yes	No	No	-	No
Participation	No	No	No	No	No	No	-
Change in Temp	No	Yes	Yes	Yes	Yes	Yes	Yes
Change in Prec.	No	Yes	Yes	Yes	Yes	Yes	Yes
Pop. Density	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years Ind.	No	Yes	Yes	Yes	Yes	No	Yes
Civ. Viol Spatial	No	Yes	Yes	Yes	Yes	Yes	Yes
Death Rate Spat.	No	Yes	Yes	Yes	Yes	Yes	Yes
Savings Spatial	No	Yes	Yes	Yes	Yes	Yes	Yes
Trade Spatial	Yes	Yes	Yes	No	No	Yes	Yes
GDPpc Spatial	No	No	Yes	Yes	No	No	Yes
Polity Spatial	No	No	No	No	Yes	Yes	Yes

Tables A10 through A16 present the simultaneous equations results for the remaining endogenous variables.

Table A10: Simultaneous Equations Estimates on Savings

	Coefficient	Std. Error	p-value
Death Rate	-1.1205	1.839	0.542
Executive Constraints	5.0428	4.780	0.291
Aid per capita	0.1838	0.938	0.845
Civil Violence	**2.1513**	1.074	0.045
GDP per capita	-0.0296	0.040	0.456
Savings spatial lag	**-75.8970**	5.705	0.000

Table A11: Simultaneous Equations Estimates on Trade

	Coefficient	Std. Error	p-value
Polity	**-2.5831**	0.041	0.000
Trade Spatial Lag	**-10.7643**	5.360	0.045
Civil Violence Spatial Lag	**-287.4631**	1.463	0.000
Savings Spatial Lag	**-25.6971**	1.440	0.000
Deathrate Spatial Lag	**81.2112**	1.716	0.000
Years Independent	**-3.0540**	0.086	0.000
Population Density	**-8.5487**	0.020	0.000
Change in Precipitation	**-7.1077**	0.595	0.000
Change in Temperature	0.5229	26.752	0.984

Table A12: Simultaneous Equations Estimates on Aid per capita

	Coefficient	Std. Error	p-value
Polity	**-2.1257**	0.047	0.000
Years Independent	-2.6351	1.908	0.167
GDP per capita spatial lag	-2.0793	2.340	0.374
Trade spatial lag	**45.1555**	6.127	0.000
Savings spatial lag	**-27.5669**	1.643	0.000
Death Rate spatial lag	**-35.8463**	1.963	0.000
Civil Violence spatial lag	**-13.9623**	1.668	0.000
Population Density	**11.6949**	0.099	0.000
Change in Precipitation	-0.5686	3.066	0.853
Change in Temperature	-0.5819	18.070	0.974

Table A13: Simultaneous Equations Estimates on GDP per capita

	Coefficient	Std. Error	p-value
Executive Constraints	**112.33**	2.73	0.000
Trade	**0.37**	0.11	0.001
GDP per capita spatial lag	**4673.58**	0.07	0.000
Savings spatial lag	**826.47**	5.56	0.000
Death Rate spatial lag	**888.47**	18.88	0.000
Civil Violence spatial lag	**-2200.65**	8.56	0.000
Years Independent	**10471.19**	0.02	0.000
Population Density	**19219.83**	0.01	0.000
Change in Precipitation	**26.08**	0.01	0.000
Change in Temperature	**97.28**	0.39	0.000

Table A14: Simultaneous Equations Estimates on Executive Constraints

	Coefficient	Std. Error	p-value
Polity Spatial Lag	**-3.4674**	0.186	0.000
GDP per capita	0.0002	0.001	0.678
Savings spatial lag	**-3.8372**	0.192	0.000
Death Rate spatial lag	**3.8846**	0.702	0.000
Civil Violence spatial lag	**-8.3760**	1.710	0.000
Years Independent	-0.4452	7.720	0.954
Population Density	-0.4544	2.127	0.831
Change in Precipitation	-1.0064	9.968	0.920
Change in Temperature	0.0146	1.722	0.993

Table A15: Simultaneous Equations Results on Polity

	Coefficient	Std. Error	p-value
Polity Spatial Lag	**-23.69**	0.19	0.000
Trade spatial lag	**5.87**	0.89	0.000
Savings spatial lag	**-13.37**	0.20	0.000
Death Rate spatial lag	**3.51**	0.32	0.000
Civil Violence spatial lag	**-31.97**	0.07	0.000
Population Density	-1.99	2.76	0.472
Change in Precipitation	-3.62	13.38	0.787
Change in Temperature	0.14	5.43	0.979

Table A16: Simultaneous Equations Estimates on Participation

	Coefficient	Std. Error	p-value
Polity Spatial Lag	**-11.09**	0.14	0.000
Population Density	**-13.97**	0.43	0.000
GDP per capita spatial lag	-3.16	7.59	0.677
Trade spatial lag	**3.89**	0.92	0.000
Savings spatial lag	**-3.80**	0.19	0.000
Death Rate spatial lag	**5.29**	0.29	0.000
Civil Violence spatial lag	**-18.41**	0.05	0.000
Years Independent	-7.37	4.90	0.132
Change in Precipitation	-1.80	13.48	0.894
Change in Temperature	-0.02	3.53	0.996

APPENDIX 10: SPATIAL AND SERIAL AUTOCORRELATION

The residuals in a panel estimation on country data may be subject to both spatial and serial autocorrelation. Denote the reduced form residual by μ_{it}. If spatial autocorrelation is present, the residual has the following form:

$$\mu_{it} = \lambda W \mu_{its} + \varepsilon_{it} \quad \text{(A7)}$$

where W is the spatial weight matrix, μ_{its} is the vector of residuals of other countries in the sample and λ is the spatial autocorrelation correction. The spatial weights in W are the inverse distances of each other country from the center of country i. The residual may also be subject to serial autocorrelation, in which case it takes the form:

$$\mu_{it} = \rho \mu_{it-1} + \varepsilon_{it} \quad \text{(A8)}$$

where ρ is the serial autocorrelation correction. To see if spatial autocorrelation was present I calculated the pairwise correlations of each reduced form residual and its spatial lag. If spatial autocorrelation is present then the correlation should be non-zero, otherwise it should be zero. I found correlations significantly different from zero in the residuals for Civil Violence, Savings, GDP per capita, Executive Constraints and Participation. I then estimated λ for each of those residuals via minimum distance and corrected them. Table A17 presents the correlations and significance of the uncorrected and corrected residuals with their spatial lags.

I examined the residuals for serial autocorrelation by initially computing the pairwise correlations of the residuals and their temporal lags. I further calculated three statistical tests for serial autocorrelation in panel data. The first is from Wooldridge (2002). It tests whether the regression coefficient on a regression of the residuals from the first difference regression on their temporal lag is equal to -.5, which it should equal if no serial autocorrelation is present. Table A18 presents the p-value from this

test; a value less than 0.05 indicates a rejection of the null of no autocorrelation. The second and third tests are adjustments of the Durbin-Watson test for panel data due to Bhargava et al. (1982) and Baltagi and Wu (1999). The Bhargava test indicates serial autocorrelation if the statistic is less than 1.81 and no serial autocorrelation if the statistic is greater than 1.90. The Baltagi and Wu (1999) test does not have a widely accepted range of critical values; I consider it to indicate autocorrelation if the statistic is less than two. These tests indicate that serial autocorrelation is present in most of the residuals. I thus calculated ρ via minimum distance and corrected the residuals for every endogenous variable except Growth.

Table A17: Correlations of Residuals with Residual Spatial Lags

	Civil Viol.	Savings	GDPpc	Ex. Const.	Participation
Reduced Form Corr.	0.11	0.18	-0.15	-0.08	-0.08
(p-value)	0.02	0.00	0.00	0.10	0.10
Corrected Correlation	0.00	0.00	0.00	0.00	0.00
(p-value)	1.00	1.00	1.00	1.00	1.00

Table A18: Tests for Serial Autocorrelation

	Base correlation	Wooldridge	Bhargava et al.	Baltagi-Wu LBI
Growth	-0.04	0.01	1.71	2.07
Death Rate	0.65	0.00	0.54	0.86
Civil Violence	0.60	0.00	0.67	0.91
Savings	0.35	0.00	1.16	1.35
Trade	0.54	0.00	0.71	1.04
Aid per capita	0.46	0.00	0.87	1.16
GDP per capita	0.37	0.00	0.94	1.30
Executive Constraints	0.41	0.00	0.89	1.30
Polity	0.48	0.00	0.78	1.17
Participation	0.41	0.00	0.93	1.28

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